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THE UNIVERSITY OF ALBERTA

PSYCHOLOGIES EXPOSED: THE UNEXAMINED  
PSYCHOLOGICAL PREMISES OF A POPULAR  
EXPLANATORY MODE IN 'DEVIANCE'

by



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The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled Psychology Exposed: The Unexamined Psychological Premises Of A Popular Explanatory Mode In 'Deviance' submitted by John L. Hagan in partial fulfillment of the requirements for the degree of Master of Arts.

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## ABSTRACT

The thesis presents a critique of symbolic interactionism as a mode of explanation in the field of deviance. The origin of this thoughtway is traced to G. H. Mead, and advocates of the perspective are known collectively as the neo-Chicagoans. Symbolic interactionism is characterized as an amalgam of mentalistic psychology and humanistic social philosophy. The emergent perspective is most popularly expressed today in a trend of thought variously designated as the "labelling perspective," the "societal reactions school," and the "underdog approach."

It is contended that the conceptualization of the interactionist perspective, as utilized in the study of deviance, is general in the extreme. Concepts are unclearly stated and operational indicators, when provided, are vague. Illustrations supporting these contentions are drawn from the work of Sutherland, Tannenbaum, Lemert, Becker, Goffman and Cohen.

It is further argued that the foundation of interactionism, when applied to the explanation of crime, includes six untested psychological assumptions. A test, via the literature, for evidence both pro and con these assumptions is provided. It is concluded that the neo-Chicagoans endorse several assumptions lacking in empirical support while advocating others whose truth is partial and vague.

When symbolic interactionism is evaluated as an explanation of one particular type of deviant behavior--opiate addiction--the conclusions continue to be dubious. Research is reviewed in assessment of Lindesmith's





thesis that opiate addiction requires linguistic and cognitive capacities thought to be distinctive of man. It is found, however, that any rat, like Everyman, can become an addict. The results thus recommend rejection of the interactionist explanation of opiate addiction.

Interactionism is accompanied by an epistemology known as "naturalism." Dedicated to a search for "essence," the procedures recommended in naturalist studies are analytic induction and ethnomethodology. It is argued that both approaches are conducive to naive and promiscuous conceptualization. Cressey's theory of embezzlement and Cicourel's study of juvenile justice are analyzed in support of these conclusions.

A final discussion concludes that the interactionists have confounded a requirement of scientific theory construction which insists that independent criteria be provided for the measurement of distinct variables. Interactionism denies the possibility of the conceptual independence of variables and demonstrates a willingness to utilize isolated measures of behavioral tendencies as indicators of "underlying" patterns of thought. Consequently, a "theoretical" system emerges that is vulnerable to tautology.





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## CHAPTER ONE

### THE NEO-CHICAGOANS: AN INTERACTIONIST

#### PERSPECTIVE IN DEVIANCE

Among explanatory styles current in the field of deviance, it is probably the interactionist perspective that has gained preference with the greatest number of North American criminologists. The origin of this perspective is often traced to George Herbert Mead and his lectures on the topic of symbolic interactionism at the University of Chicago.<sup>1</sup> Matza refers to the followers of this tradition as the neo-Chicagoans:

Though probably premature, and perhaps even a disservice to these individual sociologists, such similarity and distinctiveness warrants saddling them with a name and conceiving them as something like a school of thought. I will call them the neoChicagoans because they have revived the Chicago school's stress on direct observation and field work, have maintained and extended the relevance of the subject's view, and in a variety of other ways have indicated their appreciation of deviant phenomena and their connected enterprises. A theme that has more or less unified the neoChicagoans has been their emphasis on the process of becoming deviant and the part played by the official registrars of deviation in that process. A small but increasing number of sociologists identify with this viewpoint (1969: 37).

The impact of the Chicago tradition is such that much North American thought-taking about deviance is grounded in the assumptions of symbolic interactionism. These assumptions remain, for the greater part, untested. Accompanying these untested assumptions is a conceptualization that is general in the extreme.

The purpose of this chapter will be:

- (1) to illustrate the generality underlying the





- interactionist perspective in deviance, and
- (2) to explicate the general psychological assumptions inherent in this perspective.



## PART ONE:

### CONCEPTUAL GENERALITY IN INTERACTIONIST EXPLANATIONS OF DEVIANCE

The historical development of the interactionist perspective in deviance is characterized by conceptual generality. The terms utilized in interactionist explanations lack clear empirical referents and operational indicators are vague when provided at all. Some examples are in order:

#### Sutherland: Differential Association Theory

Edwin Sutherland is considered by many to have fathered the "modern American Sociology of deviance" (Schur, 1969: 310). Heavily influenced by early work done at the University of Chicago, Sutherland's approach combines elements of culture conflict and symbolic interactionist perspectives. As such, Sutherland's work marks the first major inclusion of interactionist assumptions into an approach to deviance. His theory of differential association thus offers a particularly important historical example of the perspective in question.

The theory of differential association has been stated by Sutherland and, later, his student Cressey in several forms. In White Collar Crime, Sutherland offers the following synoptic version:

The hypothesis of differential association is that criminal behavior is learned in association with those who define such behavior favorably and in isolation from those who define it unfavorably, and that a person in an appropriate situation engages in such criminal behavior if, and only if, the weight





of the favorable definitions exceeds the weight of the unfavorable definitions (1949: 234).

Expanded versions of Sutherland's theory are offered in the various editions of Principles of Criminology (1934; 1949; 1947; 1955; 1960; 1966; 1970). However, for our purposes, only one additional proposition need be noted. This proposition states that "differential associations may vary in frequency, duration, priority, and intensity" (1955: 78). Sutherland concludes that "In a precise description of the criminal behavior of a person these modalities would be stated in quantitative form and a mathematical ratio be reached" (1947: 7).

Sutherland's aspiration to a mathematical criminology appears unrealistic in view of the vague conceptualization contained in his theory. Cressey notes that a minimum of eight different authors, writing in seven different articles, have criticized the lack of specificity present in the theory of differential association (1960: 53). But it is Schrag who places this criticism in its most severe form:

The major defect of the "differential association" argument is not that it violates either common sense observation or the results of more refined research, but that its empirical meaning is so general and ambiguous that it defies any realistic test of validity (1955: 501).

The ambiguity of differential association theory stems from the terms of reference used in the propositional statements. How is a researcher to measure, codify, or objectify in any way "definitions of behavior"? What are the criteria of "favorable" and "unfavorable" definitions? Glaser makes this point by observing that "The phrase 'excess of definitions' itself lacks clear denotation in human experience." It should be added that the reference to "frequency, duration, priority, and intensity" of differential associations fares no better





in conveying a "clearly recognizable behavioral image" (Glaser, 1956: 438). Thus it is not at all surprising to find Glueck asking:

. . . has anybody actually counted the number of definitions favorable to violation of law and definitions unfavorable to violation of law, and demonstrated that in the predelinquency experience of the vast majority of delinquents and criminals, the former exceed the latter? (1956: 96).

The answer to this question is negative, if we are speaking solely in terms of Sutherland's original theoretical formulation. The explanation of this situation is the simple fact that it is impossible to operationalize the concepts of this theory without reformulating the theory itself. Several reformulations of the theory have been attempted and, consequently, several empirical tests have been carried out. However, this is not our present concern. The point made here is that Sutherland's original and standing statement of the theory is general to the point of empirical uselessness.

#### Tannenbaum: The Dramatization of Evil

Shortly after Sutherland published his Principles of Criminology, Frank Tannenbaum finished Crime and the Community (1938). Here again we find an emphasis on the process of learning through symbolic interaction. It is emphasized that conduct is learned in the sense that it is a response to a situation made by other people.

It is here that we must look for the origin of criminal behavior. . . . What one learns to do, one does if it is approved by the world in which one lives (1938: 11).

Tannenbaum sees this learning process as taking place over the period of a criminal career. During this space of time the acts of the individual take on generalized "meaning."



There is a gradual shift from the definition of the specific acts as evil to a definition of the individual as evil. . . (1938: 17).

But each step in this "process" is not of equal import, and it is here that Tannenbaum anticipates what was later to be rediscovered as the labelling perspective.

The first dramatization of the "evil" which separates the child out of his group for specialized treatment plays a greater role in making the criminal than perhaps any other experience (1938: 19).

Beyond this, Tannenbaum postulates that the "dramatization of evil" has a striking psychological impact on the individual (1938: 19-20). It is proposed that a process of "self-identification" as criminal is set in motion. Tannenbaum is thus interpreted as saying that the imposition of a societal label sets in motion a process that yields a criminal self-concept.

With Tannenbaum's perspective summarized, it becomes possible to offer some critical observations. It would seem that this approach suffers from several of the same difficulties noted for differential association theory. For example, Tannenbaum proposes that the individual initially engages in criminal behavior because such action yields approval in the actor's world. But how does one, for measurement purposes, objectify approval? What are the indicators of approval? When a subcultural group refers to an event or person as "bad," "mean," or "freaky," is this approval or disapproval? Certainly the linguistic referents of this concept are ambiguous at best.

A further problem develops with a notion of a "first dramatization of evil." While Tannenbaum considers such an event to be a critical stage in the development of a deviant self-concept, nowhere do we find





objective criteria for recognizing this event. To avoid tautology, Tannenbaum must supply an indicator of dramatized evil separate from a change in self-concept. What is this indicator: police interrogation, arrest, court appearance, conviction?<sup>2</sup> Tannenbaum seems content to stop short of the necessary specification of terms. In their stated form, Tannenbaum's propositions are plausible but untestable.

#### Lemert: The Tolerance Quotient And Secondary Deviance

The interactionist perspective, applied early in the study of deviance by Sutherland and Tannenbaum, was to slip in popularity during the decade following its formal introduction. However, with the appearance of Lemert's Social Pathology (1951), new life was given to this thoughtway. One scholar is moved to reflect on the emergence of a "new deviance analysis" (Schur, 1969: 310). Thus Lemert and his successors have been grouped under several designations: "the societal reactions school," "the underdog philosophy," and most commonly "the labelling perspective." However, it is argued here that these new titles simply represent a modern version of an old story. This theoretical perspective, by any other name, is still grounded in the assumptions of symbolic interactionism.

The work of Edwin Lemert (1951; 1967) marks an attempt to clarify and extend previous concepts. His aspiration seems consistent with the distinctive features of scientific theory construction.

The final gauge of all theory is the extent to which it meets the empirical test, i.e., how well it is borne out by the evidence, or how well it explains the phenomena we are interested in. Beyond this, there is the ultimate objective of all theory to avoid ad hoc explanations. This means that if the theory achieves a true generality it will permit generalizations and predictions covering any of the data falling





into the field of behavior which has been marked off by the theory (1951: 25).

Certainly Lemert is on the right track in his characterization of scientific theory. However, like his predecessors, Lemert's efforts may be marked more by aspiration than achievement; illustrations follow:

Where Sutherland and Tannenbaum have explained human action by reference to "favorable definition" and "approval," Lemert introduces the terms "satisfaction" and "pleasantness."

. . . people beset with problems posed for them by society will choose lines of action they expect to be satisfactory solutions to problems. If the consequences are those expected, the likelihood that the action or generically similar action will be repeated is increased. If the consequences are unsatisfactory, unpleasant, or make more problems than they solve, then the pattern of action will be avoided (1967: 54).

As with "favorable definition" and "approval," "satisfaction" and "pleasantness" suggest neither clear nor objective empirical referents. This lack of specificity eliminates the possibility of prediction and renders virtually any human act of deviation understandable in retrospect. By now there is the suspicion that a recurring characteristic of the interactionist perspective in deviance is, despite Lemert's aspirations, the reliance on ad hoc explanation, and an avoidance of predictive test.

The best known of Lemert's contributions to the interactionist perspective are his conceptualizations of the tolerance quotient and secondary deviation.<sup>3</sup> The concept of tolerance quotient is best left in Lemert's own words:

The complex of variables of which the societal reaction is a function can be summed up and expressed in the concept of the tolerance quotient. As formally stated, the concept appears to be a quantitative expression of deviation and the



willingness of the community to accept or reject it.

This concept suggests to us the possibility of handling sociopathic deviation and the reaction to it in the community as a mathematical ratio or a fraction. The top of our fraction, the numerator, will be a measure of the amount of some disapproved conduct in a stated locality. The denominator will measure the degree of tolerance which the people in this locality have for the behavior in question. It is assumed that both numerator and denominator in this ratio may change, either by increasing or decreasing, or that one may change in either of these ways while the other remains unchanged. When, as a result of such changes, this ratio reaches a certain point, let us say 1 to 1, the people in the locality will begin to do something about the deviant behavior. . . . This is the critical point in the tolerance quotient (1951: 57).

Lemert has shrouded his notion of the tolerance quotient within the cloak of a deceptively simple mathematical formula. The numerator of Lemert's ratio refers to "the amount of some disapproved conduct in a stated locality." Does Lemert here refer to the amount of official crime reported in a locality or to that more elusive quantity known as deviance? One would guess that Lemert intends the latter with its attendant lack of specific measures. But the denominator of Lemert's ratio, "the degree of tolerance which the people in this locality have for the behavior in question," is even more difficult. How does one measure the willingness of a community to tolerate "deviance"? To avoid tautology, one must here use a measure separate from change in crime rate. Lemert supplies no answer to this problem but rather seems content to remain within the secure regions of conceptual generality.

Lemert has also proposed the conceptual construct of secondary deviation. The purpose of this concept is to distinguish two varieties of deviance. Primary deviation refers to the initial acts of the individual which call out the societal response; its psychic effects are





said to be minimal.

. . . . Primary deviation has only marginal implications for the status and psychic structure of the person involved (1967: 40).

Thus Lemert here seems to draw a picture of the deviant similar to that drawn by Tannenbaum before the dramatization of evil. The deviant's self-concept has yet to be altered by the reaction of "other." Secondary deviance, however, refers to the problems that arise from the societal reaction to the initial deviance. These are generally problems of morale and,

They become central facts of existence for those experiencing them, altering the psychic structure, producing specialized organization of social roles and self-regarding attitudes (1967: 40-41).

When a person begins to employ his deviant behavior or a role based upon it as a means of defense, attack, or adjustment to the overt and covert problems created by the consequent societal reaction to him, his deviation is secondary. Objective evidences of this change will be found in the symbolic appurtenances of the new role, in clothes, speech, posture, and mannerisms, which in some cases heighten social visibility, and which in some cases serve as symbolic cues to professionalization (1951: 76).

Lemert here speaks of dramatic differences in "psychic structure" and "self-regarding attitudes" that allegedly result from the societal response to the initial deviance. But we are left to wonder what the behavioral differences may be. Does deviant behavior change objectively after the societal response? Lemert supplies no answer here. In fact, the only objective evidence of change suggested are alterations of "clothes, speech, posture, and mannerisms." Are these significant alterations? And, even more to the point, are these alterations consequent to societal response?

What is lacking in Lemert's scheme is a taxonomy of personality



types and a taxonomy of societal reactions. It might then be possible to correlate these categories and discover if there are any statistical regularities involving objective transformations of deviant behavior consequent to societal reaction. But Lemert does not take us in the direction of this goal. Instead, Lemert would seem to prefer operating on a level of generalization that speaks of conceptual distinction without any obvious behavioral difference. Specificity is again sacrificed in favor of the comforts of generality.

### Becker: Outsiders

The publication of Howard Becker's Outsiders is coincident with a rapid politicization of deviance analysis in North America. It is here argued that the "deviance analyst" might serve a useful function by presenting the viewpoint of society's "other side." In this spirit, Becker argues the passive innocence of our culture's unfortunates.

The deviant is one to whom the label has successfully been applied; deviant behavior is behavior that people so label (1963: 9).

But Becker's "truth" remains so only in a very general sense. It ignores the fact that behavior can be more than its label. Where Becker indicates that behavior is characterized only by the variety of its definitions, we would argue that much behavior is further characterized by its objective content. Certainly man imposes order on his phenomenal world by abstracting and generalizing, and an essential tool in this process involves the use of labels, but this does not mean that reality is "all in our heads" and that as a result nothing has an independent existence "out there." What happens "out there" usually has a distinct relationship to what is experienced "in our heads"; to ignore this fact





is to stretch the subjectivist's insight into the sophist's deception.

Perhaps a useful means of avoiding this error in the study of deviance is to distinguish those societal labels that have "grown naturally" from others that have been purposefully enacted (cf. Nettler, 1970b: 10-11). Becker's discussion of deviance seems slanted towards those forms of deviance labelled legislatively in the latter manner. However, it is also important to attend to those more permanent forms of deviance whose labels reflect a crecive historical background. The distinction is expressed well in the separation of deviant behaviors mala prohibita from those characterized as mala en se. While the interpretation, relative gravity, justification, and, hence, societal response to both types of deviance will vary with alterations in their legal labels, this proposition will be of critical importance for only the former class of deviant behaviors. Among the latter class of deviant activities, those behaviors whose social definitions have a crecive history, a change in legal label will have only a minor immediate impact. Thus deviant behavior mala en se will usually persist as a social problem despite a change in its legal label.

Among Becker's other offerings to the field of deviance is the notion of a sequential model--". . . a model that allows for change through time" (1963: 22). Becker's promotion of such a model suggests a lack of appreciation for the complexities of causation;<sup>4</sup> in addition, it also results in a reintroduction of conceptual generality. Two terms incorporating this generality are "career" and "career contingency."

A useful conception in developing sequential models of various kinds of deviant behavior is that of career. . . the concept refers to the sequence of movements from one position to another. . . . Furthermore, it includes the



notion of "career contingency," those factors on which mobility from one position to another depends (1963: 24).

While the concept of career is certainly not by nature lacking in specificity, it becomes so in the writings of Becker. To illustrate this charge, we need go no further than the beginning stages in Becker's conceptualization of the deviant career. Becker begins his analysis by asking the following question:

The first step in most deviant careers is the commission of a non-conforming act, an act that breaks some particular set of rules. How are we to account for the first step? (1963: 25).

Becker answers this question by first criticizing a predominant explanation of primary deviance: "deviant motivation." Certainly the concept of motivation is often vague beyond empirical usefulness and well deserving of criticism. However, in Becker's analysis, motivation is replaced with the even more vague notion of "commitment."

Something of an answer to this question may be found in the process of commitment through which the "normal" person becomes progressively involved in conventional institutions and behavior. In speaking of commitment, . . . I refer to the process through which several kinds of interests become bound up with carrying out certain lines of behavior to which they seem formally extraneous. . . .

. . . the normal development of people in our society (and probably in any society) can be seen as a series of progressively increasing commitments to conventional norms and institutions. . . .

This suggests that in looking at cases of intended non-conformity we must ask how the person manages to avoid the impact of conventional commitments (1963: 27-28).

Becker's notion of commitment seems to rely on such general definitional terms as involvement and interest. It is extremely difficult to pin down the intended meaning of "interest," but "involvement" may mean active participation. However, this seems largely tautological:





A conformist (non-deviant) is distinguished from a non-conformist (deviant) by his active conformity. In short, a conformist is defined by conformity.

Certainly what Becker says is redundantly true, but it doesn't take us very far.

### Goffman: Stigma

Combining the core assumptions of the interactionist perspective with sympathies similar to the labelling theorists, Erving Goffman makes his own unique contribution to the social-psychology of deviance. In Stigma, Goffman argues for a more inclusive area of analysis.

I have argued that stigmatized persons have enough of their situations in life in common to warrant classifying all these persons together for the purpose of analysis. An extraction has thus been made from the traditional fields of social problems, race and ethnic relations, social disorganization, criminology, social pathology, and deviancy--an extraction of something all these fields have in common (1963: 146-47).

Like Becker, Goffman uses "career" as his central concept. But in Goffman's hands the concept becomes more complex.

One value of the concept of career is its two-sidedness. One side is linked to internal matters held clearly and closely, such as image of self and felt identity; the other side concerns official position, jural relations, and style of life, and is part of a publically accessible institutional complex. . . .

. . . the main concern will be with the moral aspect of career--that is, the regular sequence of changes that career entails in the person's self and in his framework of imagery for judging himself and others (1961: 127-28).

The key terms here are self-image and identity. While these terms themselves are extremely vague, the usage of the "moral career" concept becomes even more troublesome. The concept is so general that it



becomes difficult for even Goffman to avoid the pitfalls of tautology.

Persons who have a particular stigma tend to have similar learning experiences regarding their plight, and similar changes in conception of self--a similar "moral career" that is both cause and effect of commitment to a similar sequence of personal adjustments. (1963: 32, emphasis added).

It will be remembered that Goffman has already defined moral career in terms of "the regular sequence of changes in the person's self." Certainly this is no different from a "sequence of personal adjustments." Thus the phenomenon to be explained (a "sequence of personal adjustment") is identical to that which explains it (the "moral career" concept). Goffman's central concept is every bit as broad as the phenomenon he attempts to explain. Conceptualization in such general terms carries with it the potential threat of tautology.

Several other concepts, each decreasing the possibility of prediction and empirical test, are introduced into Goffman's discussion of stigma. Among these is that concept referred to as "personal identity."

By personal identity, I have in mind. . . two ideas--positive marks or identity pegs, and the unique combination of life history items that comes to be attached to the individual with the help of these pegs for his identity (1963: 63).

To this already nominalistic conception of personality is added the notion of ambivalence.

Given that the stigmatized individual in our society acquires identity standards which he applies to himself in spite of failing to conform to them, it is inevitable that he will feel some ambivalence about his own self (1963: 106).

It is only to be expected that this identity ambivalence will receive organized expression in the written, talked, acted, and otherwise presented materials of representatives of the group (1963: 108).

The end result of Goffman's conceptualization is a construct so





broad in its range of behavioral possibilities that an empirical test is unfathomable. Personal identity is seen as unique, stigmatized personal identity inevitably yields ambivalence, and ambivalence results in sharply contrasting behaviors. In such a scheme, all pasts become "understandable," and no futures predictable. We are still left to wonder what the stigmatized individual will do next.

### Cohen: The Deviant Act

If Goffman leaves us confused, the work of Albert Cohen offers no consolation. Having studied early in his career with Edwin Sutherland, Cohen has long been familiar with the interactionist perspective in deviance. However, it was not until more recent years that the assumptions of symbolic interactionism emerged as an orienting feature of his work. The emergence of this orientation is signalled as follows:

Another starting point for a theory of deviant behavior grows out of the social theory of George Herbert Mead. This starting point is the actor engaged in an ongoing process of finding, building, testing, validating and expressing a self (1965: 12).

To study this "process," Cohen suggests the use of a "tree" model. Such a model consists of alternative paths of behavior that fall into a "branch-like" pattern.

The theory may, of course, contemplate more than one pathway to deviance, or different pathways leading to different kinds of deviant actions as well as to conformity (1966: 45).

If one is bewildered by the behavioral possibilities of the above model, then even more confounding is Cohen's concluding remark:

The test of interaction process theories is how well observed pathways correspond to those which the theory would predict (1966: 45).



Cohen would have us overlook the fact that interactionist theories seldom, if ever, do predict. The generality inherent in the interactionists' processual theories suits them for ex post facto explanation. One cannot reasonably expect both to describe accurately the full range of human group life, in all its complexity, and at the same time to entertain hopes for predictive tests. An inclusive description of human complexity implies generality; a predictive test requires specificity. Cohen comes close to the point in his final remarks.

. . . interaction process theories come closest to making provision, somehow, for the full range of relevant considerations. However, precisely because they come closest to recognizing the full complexity of the real world, they are most difficult to formulate in neat, tight, logical, and testable systems. Perhaps the reason for the small number of serious attempts to formulate such theories is that the task is so forbidding (1966: 45).

The task may not only be forbidding; it may also be impossible. No matter how hard we try, science simply will not answer all our questions--Lundberg notwithstanding. This is not a derogation of the scientific thoughtway; we intend to argue that a scientific approach is, for our purposes, the preferable means of thought-taking. However, to play at the game of science, and to win its rewards, we must begin with reduced expectations--particularly if our playing field is the world of social reality.

Life in all its fullness is too complex to be understood or controlled; we have to single out certain important aspects which are relevant to the phenomena in which we are interested, in the hope that this will enable us to formulate theories regarding these limited fields, and gain even limited understanding and control. We may then become more ambitious and extend our search, attempting to cover larger areas, to increase our understanding, to integrate our knowledge with that derived from other fields, and thus slowly seek to reduce the area of our ignorance (Eysenck, 1964: 179).





It would seem that the neo-Chicagoans have missed this lesson of science.

There is, however, an even more serious problem. Despite the complex formulas contrived by the neo-Chicagoans, despite the mathematical jargon often incorporated in their prose, despite the acknowledged importance of predictive empirical tests--such tests seldom take place.

One is alerted to the possibility of hoax, to the possibility of being persuaded that scientific theories may explain without incurring the probative liability of prediction. If the principles adduced to explain a past cannot be used in the anticipation of a future, then any plausible story will do and the test of the explanation remains the contentment of curiosity. If there is no pattern of experience to be described, then there are no "lessons of history" and its study becomes dilettantism. It seems to involve a contradiction, albeit a protective one, to maintain that scientific theories may explain without making a contribution to forecast (Nettler, 1970b: 127-28).

If the interactionist perspective in deviance is to entertain the status of science, then it must meet the challenge of prediction.



## PART TWO:

### UNEXAMINED PSYCHOLOGICAL PREMISES IN INTERACTIONIST EXPLANATIONS OF DEVIANCE

Accompanying the generality inherent in the interactionist perspective in deviance are a series of assumptions. These assumptions take the form of psychological premises that have been accepted by many sociologists without test. It is contended here that the uncritical acceptance of such assumptions may be a major source of the tautological explanations that infect many social-psychological studies of deviance.

The purpose of the following discussion will be to illustrate and explain several of the working assumptions of symbolic interactionism so that they become amenable to empirical test. An actual test, via the literature, for evidence both pro and con these assumptions, will be carried out in Chapter Two.

There are at least six working assumptions of symbolic interactionism as applied to the explanation of crime:

#### Assumption I: Psychological Differentiae Do Not Exist In a Manner Relevant to the Production and Expla- nation of Criminal Behavior

The interactionist perspective not only ignores, but refuses the determining influence of psychological differentiae in criminogenesis. Simple neglect of such psychological variables, in the interest of parsimonious theory construction, may be defensible. However, denial of the existence of such differentiae goes beyond the defense provided by





heuristic purpose. Nevertheless, evidence of such a denial is found in the following:

Sutherland: The fact which stands out most clearly from the organized research studies which have been conducted by scholars representing different schools of thought is that no trait of personality has been found to be very closely associated with criminal behavior (1955: 135).

Becker: Insofar as the category lacks homogeneity and fails to include all the cases that belong in it, one cannot reasonably expect to find common factors of personality or life situation that will account for the supposed deviance (1963: 9).

Lindesmith: Within criminal groups there are found more or less the same individual psychological variations as in other occupational groups (1956: 664).

Tannenbaum: The assumption that crime is caused by any sort of inferiority, physiological or psychological, is here completely and unequivocally repudiated (1938: 22).

The complete rejection of psychological difference as an influence in the origin of crime renders the interactionist perspective not merely incomplete--but also in danger of error. If psychological variables do exist that might distinguish classes of criminals from non-criminals, then the interactionists have built their explainway on a faulty foundation. Evidence for or against this conclusion is to be found in Chapter Two.

Assumption II: "Taking the Deviant's Viewpoint" Is An Effective Means of Gaining An "Understanding" Of His Behavior

The interactionist explicator "feels" that he is able to "take on the viewpoint" of the deviant and, thereby, to "understand" his behavior. Howard Becker traces the source of this assumption to the work of George Herbert Mead.



We must always look at the matter from someone's point of view. The scientist who proposes to understand society must, as Mead long ago pointed out, get into the situation enough to have a perspective on it (1967: 245, emphasis added).

The importance of "role-taking" to the "understanding" of deviant phenomena is underlined by Becker. It is argued that to study deviant behavior we must "take on" either the deviant or non-deviant role.

If we study the processes involved in deviance, then, we must take the viewpoint of at least one of the groups involved, either of those who are tested as deviant or of those who label others as deviant (1963: 172).

There is difficulty in knowing what is meant by Becker's use of the phrase "taking the viewpoint." No indicators of such a procedure are specified, and the benefits of their use are therefore unclear. There is additional difficulty in knowing the interactionists' intended meaning of the term "understanding." Again, indicators of such a condition or state are left unspecified, with resulting confusion regarding the occasions of its achievement.

Fortunately, efforts in other quarters have facilitated concrete discussion of the terms discussed above. Thus researchers have commonly used judge's predictions of target other's responses as an indicator of "role-taking" activities, while additionally using the correspondence of these predictions with actual responses as an indicator of the accuracy of the "knowledge" or "understanding" gained through these procedures. Taken in whole, such studies seek to provide a generalized measure of the "ability to take the role of the other." Sarbin provides a concise description of this experimental approach as it is commonly utilized.

Ability to take the role of the other has frequently been investigated by requiring one person to predict the responses of another person on a series of attitudinal





items or on a personality test. How well such prediction is accomplished is usually determined by comparing the predictions with the actual responses of the other person. . . (1968: 516).

Although there are serious logical difficulties inherent to this type of research design (Gage and Cronbach, 1955), modifications in the design allowing systematic control of relevant variables (for example, see Blanchard, 1967) have made it possible to eliminate many of the methodological artifacts involved. Results of such studies are, then, of considerable importance to questions surrounding the process of role-taking.

Studies of the type described by Sarbin will be discussed in Chapter Two. For the moment, it will be sufficient to note several aspects of interpersonal judgment involved in role-taking activities that tend to jeopardize the accuracy of the results. The first of these "judgmental effects" to be noted is the "halo effect." This effect consists of the tendency to predict specific responses of the target other in terms of a general impression of goodness or badness. A second effect to be considered is referred to as the "logical error." The bias here consists of the tendency to forecast responses of other in accord with preconceptions of 'what' can be expected to be accompanied by 'what else.' A third judgmental tendency is known as the "leniency effect" and is characterized by the inclination to rate the target other high on favorable response items and low on unfavorable response possibilities. Each of the above judgmental tendencies is a potential error-producer in the formation of interpersonal judgments, and the interactionists are thus susceptible to a variety of perceptual difficulties when engaged in the process of "taking the other's viewpoint." The problem is one of



possible naivety in the judgmental process. Obviously, all perceptions so gained by the interactionists will not be in error; however, the problems specified still remain, and there is a need for their acknowledgment.

The problem, when stated in its broader context, amounts to this: Is the procedure of "role-taking" the most effective means of "knowing" about other's behavior? One is provided with a basis for answering this question when it is assumed that a measure of "knowing" is the ability to make accurate predictions regarding the subject(s) under investigation. With reference to the relative utility of "role-taking," comparisons of the clinician's use of empathetic methods and the actuary's use of statistical tables, offer probably the most relevant answer to the question posed. Such comparisons, involving attempts to predict delinquent, criminal, and recidivist behaviors, will be discussed in Chapter Two.

### Assumption III: Behavior is Mediated by Thought

Central to the interactionist perspective in deviance is the assumption that behavior is mediated by thought. From the outset it is acknowledged here that behavior is to some degree influenced by thought. Nettler suggests that, "Intuitively there is a rightness about it. Each man knows from his own experience that his 'attitude' makes a difference" (1970b: 55). Thus there is a need to exercise caution in our criticism of this assumption; we repeat that the assumption is fruitful in its expression of a partial truth. Yet:

True. But only "true enough." For when one thinks about "how a man thinks," the phrase becomes vague, its measures





imperfect, and the correlates with other acts less than the phrase has led one to expect (Nettler, 1970b: 56).

There are, then, basic problems in the neo-Chicagoans' efforts to relate 'what one thinks' to 'what one does.' Not the least of these difficulties accrues with the necessity of specifying indicators for the "mediational process" which is assumed to link thought to behavior.

The neo-Chicagoans couch their discussions of thought in conceptual references to "definitions of the situation."

Sutherland: A person becomes delinquent because of an excess of definitions favorable to violation of law over definitions unfavorable to violation of law (1955: 78, emphasis omitted).

Becker: . . . we look to the process by which the common definition arises. This is, with increasing frequency, referred to as the process of labelling. People attach the label 'deviant' to others and thereby make deviants of them (1964: 2-3).

Goffman: When we allow that the individual projects a definition of the situation when he appears before others, we must also see that the others, however passive their role may seem to be, will themselves effectively project a definition of the situation by virtue of their response to the individual and by virtue of any lines of action they initiate to him (1959: 9).

Lindesmith: The individual is rather continuously engaged in communication with himself as he enacts his roles. To begin with, he must know or try to figure out what his part in the situation is or should be. . . . The individual's conception of his role has a controlling function over responses in that it determines which specific responses will be evoked (1956: 384).

Although the conceptualization is vague at best, the interactionists here seem to be saying that an individual's definition of the situation determines his behavior pattern. Thus: deviant definitions result in deviant behaviors. The difficulty with this proposition inheres in the fact that behavior, for the interactionist, is also often



a matter of definition. Increasingly, variations of self-report procedures have become the means of indicating deviant behavior. Since a self-report is obviously a definition of the situation, the latter part of our initial proposition is potentially identical to the first part: deviant definitions result in deviant definitions--a seemingly tautological statement.

The interactionists' attempts to relate cognition to behavior seem to suffer a disregard for the basic requirements of scientific theory construction. The criteria chosen to operationalize thought and behavior, as well as the mediation process that links thought to behavior, do not reflect the assumed conceptual independence of these terms. In other words, thought, behavior and the mediation process often seem to end up being measured as the same thing. The avoidance of tautology requires that each of these terms be measured independently. The use of so vague a term as "definition of the situation" and the reliance on verbal reports confounds this requirement.

Assumption IV: Perceptual Discrimination Is Regulated  
By the Manipulation of Linguistic Symbols

In spite of the difficulties inherent in the neo-Chicagoans' discussions of the thought process, there remains the possibility of rendering their position testable. The frequent use of such terms as "interpretation," "meaning," and "definition" suggests that reference is intended to the ability to make perceptual discriminations. This ability to make perceptual discriminations is further assumed to be dependent upon the capacity to manipulate corresponding verbal symbols. Thus an equation is postulated between the act of discrimination and the





manipulation of a corresponding set of linguistic symbols.

To bind discrimination so closely to the manipulation of linguistic symbols would seem to imply a rather short-sighted view of the process of human perception. Yet George Herbert Mead was quite explicit in addressing himself to this point.

Language does not simply symbolize a situation or object which is already there in advance; it makes possible the existence or the appearance of that situation or object, for it is a part of the mechanism whereby that situation or object is created (1934a: 78).

The question to be asked of such statements is: "How much perceptual discrimination is regulated by the postulated verbalization?"<sup>5</sup>

The purpose of posing such a question is not, of course, to say "all wrong" and "no truth." Certainly the manipulation of language symbols must play some role in the process of perceptual discrimination. However, to assert such truths with the adamancy characteristic of the neo-Chicagoans' discussions is partial and productive of a systematic bias.

The empirical basis for such a "linguistic bias" will be evaluated in Chapter Two. Among the evidence presented will be a review of research investigating perceptual discrimination in "non-language-able" contexts.

Assumption V: An Actor's Verbalizations, As Indicators of Thought, Explain His Behavior

The interactionist perspective suggests that we can know how others "think," and therefore "explain" how others behave, by attending to what they say. Lindesmith announces this assumption in a concise fashion:

. . . in order to explain why people do what they do we must know how they think. The chief source of information about how people think is what they say (1956: 9).



Lemert has also carried this assumption into the study of deviance:

We hold that deviation in covert symbolic processes can be studied empirically through language which is conceived as behavior (1951: 35).

Methodological procedures, based on this assumption, have been carried into the field by researchers of the interactionist persuasion. Becker's study of the dance musician provides an example of such an approach:

I seldom did any formal interviewing, but concentrated rather on listening to and recording the ordinary kinds of conversation that occurred among musicians. Most of my observation was carried out on the job, and even on the stand as we played. Conversations useful for my purpose often took place also at the customary "job markets" in the local union offices where musicians looking for work and band leaders looking for men to hire gathered on Monday and Saturday afternoons (1963: 84).

Operating on the premise that people say what they "think," and assuming that "thoughts" explain behaviors, researchers have continued to follow the procedures illustrated in Becker's study.

If we take as the interactionists' meaning of "explanation" that (1) their attention to verbalizations allows them to isolate causes, and/or (2) make better predictions with reference to other measures of behavior, then their premise is testable. In this context, we are reminded of the pessimist's warning that people irregularly say what they mean, and, only with unknown regularity, do what they say. Such advice at least seems worthy of empirical test, to be attempted in Chapter Two.

#### Assumption VI: "Other's" Definition of Subject Determines Subject's Behavior

This final assumption is one that is consistently utilized by those in the "labelling school." Lemert's development of the secondary deviance concept is perhaps a classic implementation of this assumption.





The sequence of interaction leading to secondary deviance is roughly as follows: (1) primary deviation; (2) social penalties; (3) further primary deviation; (4) stronger penalties and rejection; (5) further deviation, perhaps with hostilities and resentment beginning to focus upon those doing the penalizing; (6) crisis reached in the tolerance quotient, expressed in formal action by the community stigmatizing of the deviant; (7) strengthening of the deviant conduct as a reaction to the stigmatizing and penalties; (8) ultimate acceptance of deviant social status and efforts at adjustment on the basis of the associated role (1957: 77).

Lemert's sequential model is troublesome in several ways. First, there is nothing in the way of overt behavior noted to distinguish secondary from primary deviance. Both seem indicated by societal or self-definition of behavior as deviant. Second, the "social penalties" and "stigmatization" mentioned are again indicated by nothing other than societal or self-definition. Finally, the "ultimate acceptance of deviant social status" is yet another concept indicated by societal or self-definition, or more accurately, it is a concept indicated by the merging of societal and self-definition. Thus this final assumption of the interactionist perspective in deviance would seem to lead once more into the problems of tautological thought.

It has been shown that the neo-Chicagoans utilize six working assumptions characteristic of symbolic interactionism. Many of these assumptions may, in the past, have been fruitfully accepted in the pursuit of knowledge. However, there also arrives a time for testing assumptions and questioning the merit of their endurance. In this spirit, the six working assumptions of the interactionist perspective in deviance will now be assigned the status of unresolved questions.

Empirical answers, limited by the boundaries of current research, are to be found in Chapter Two.



## FOOTNOTES

<sup>1</sup>Transcriptions of these lectures have been published (Mead, 1934a). An early article by Mead (1918) also provides an anticipation of what is now known in the field of deviance as the "labelling perspective."

<sup>2</sup>Assuming the choice of any one of these indicators, it is interesting to note a recent study by Maher. Data from this study indicates that ". . . a series of experiences common to delinquents such as arrest and a court appearance does not seem to change the offender's self concept. . ." (1968: 220).

<sup>3</sup>For the original conceptualization of the tolerance quotient see Van Vechten (1940).

<sup>4</sup>Among other things, Becker's simplistic model ignores the possibility of a plurality of causes and of the interaction of variables.

<sup>5</sup>In addition, there is a question as to the meaning of the term "symbol," as used by the interactionists.

It is a useful distinction to note that a symbol actually performs only a sign function when it calls out a completed interpretation of an event or fact. The performance of a symbol function, on the other hand, involves calling out a concept of the event or fact, which must then undergo the process of interpretation. In this manner, Skidmore (1969) has demonstrated that George Herbert Mead often uses the term "symbol" where the term "sign" would be a functional equivalent.

This subtle distinction is most important to the idea of communication in Mead's theory. If the symbol calls out an interpretation of the thing symbolized, then the symbol is really performing a sign-function. . . . This indeed seems to be what Mead means, at least most of the time (1969: 285).

This lack of conceptual clarity that characterized Mead's work has also been carried over into the work of the neo-Chicagoans in the field of deviance. As an example, in his discussion of "stigma symbols," Goffman fails to make a meaningful distinction between "sign" and "symbol."

Some signs that convey social information may be frequently and steadily available, and routinely sought and received; these signs may be called "symbols" (1963: 43).

Goffman seems to distinguish a symbol from a sign primarily on the basis of the frequency of its use. This would appear to be only a





numerical, and not a functional, difference. In fact, one could argue that, as a symbol becomes routinized, it takes on the character of a sign. That is, the symbol begins to evoke reflexive response--behavior without "thought." As in the work of Mead, Goffman's use of the terms "symbol" and "sign" seems to be functionally indistinct.

There is, then, confusion surrounding the interactionists' use of the terms "symbol" and "sign." The current discussion will retain the interactionists' use of the term "symbol"; however, in accord with Skidmore's argument, it is acknowledged that much of the intended reference may be to the performance of sign functions. Fortunately, this distinction will not be crucial to the current discussion.



## CHAPTER TWO

### A TEST OF FAITH

The present chapter attempts to summarize empirical research relating to the several working assumptions of the interactionist perspective in deviance. In this manner, assumptions previously accepted as articles of faith now become hypotheses amenable to empirical test.

#### Hypothesis I: Psychological Differentiae Do Not Exist In a Manner Relevant to the Production and Expla- nation of Criminal Behavior

While the neo-Chicagoans have characteristically remained skeptical of efforts to document the relationship between various psychological differences and deviant behavior, other criminologists have remained more receptive to this avenue of research. By now, facts are available for evaluating the resulting discrepancy in viewpoints.

Early efforts to locate psychological differences among deviant populations focussed on the factor of intelligence. Clara Cooper (1960) has provided perhaps the most extensive review of studies attempting to measure the relationship between intelligence and deviant behavior.<sup>1</sup> This review incorporates 176 studies published prior to January 1, 1928; represented in these studies are a total of 13 different countries. Cooper acknowledges that findings during this period were frequently exaggerated. Nonetheless, she concludes that delinquency tends to be much more common among the feeble-minded than among people in general and that mental deficiency is very much more prevalent among delinquents





than among the public-at-large. The implications of these findings are two-fold:

. . . . The facts firmly established by the study are, first, the existence of a direct relation between delinquency and mental inferiority, . . . ; and, second, the very great variation in the degree of relationship that may normally be expected in restricted groups, variously selected, and studied by different investigators using diverse methods. Caution in applying these findings is, therefore, the only wise procedure (1960: 202).

What Cooper and others of this period seem to have been recommending was a cautious and flexible attitude toward what were as yet still quite tentative findings (cf. Wallin, 1922: 34).

Sutherland indicates a less patient attitude in his review of 350 American psychometric studies conducted in the period from 1910 to 1928. Using findings from the median study of this period as a representative figure, Sutherland notes that the estimated proportion of feeble-minded persons among delinquents decreased from 50 to 20 per cent. Mannheim suggests that if Sutherland had repeated his survey thirty years later, he would have found that, with an improvement in tests, the proportion of markedly defective delinquents had further decreased (1965: 275).

An analysis of studies completed between 1930 and 1940, however, presents a slightly different picture. Metfessel and Lovell (1942) argue in their review of studies completed during this period that the concept of intelligence, particularly as it relates to deviant behavior, has been unfairly treated. Noting that it is the vaguely stated correlates of deviant behavior that are presented with the most conviction, the authors allege that defining the relationship of intelligence to deviance in specific terms has had the exaggerated consequence of almost



completely undermining the relationship's credibility:

When factors are made specific and subject to rigorous scrutiny, less assurance is shown that any given one is significantly related to crime. On this account, intelligence as a factor probably is underevaluated, and vaguer concepts, such as "bad parents," probably are overevaluated (1942: 153).

The final conclusion emerging from this view of intelligence testing points out that, while mental deficiency is not regarded as important a cause of crime as it was in the early days of such research and while there may be considerable disagreement as to just how important this correlate is, the wide majority of studies still do support placing the "typical delinquent" in the "dull normal class" of intelligence.

Herman Mannheim (1965), in a more recent discussion of the literature in this area, highlights the work of Mary Woodward. Woodward summarizes a number of American studies completed between 1931 and 1950. Using an approach similar to Sutherland's, she concludes that the mean I.Q. of delinquents studied had increased in this period from 71 to over 92. However, it is to be noted that this figure still leaves a difference of ten points in favor of non-delinquents. Mannheim's interpretation of Woodward's findings supplies selective reinforcement for the view expressed by Metfessel and Lovell twenty years before.

Here as in many other fields of criminology there is a danger that insight into the exaggerations and short-comings of previous generations may lead to errors of the opposite kind: over-estimation of the role of subnormality may be replaced by under-valuation (1965: 278).

The most important point to be made with reference to the relationship between intelligence and criminality emerges when the type of crime is considered. Thus, as early as 1939, Tulchin reported the following findings from an exhaustive study utilizing intelligence tests





in the state prisons of Illinois:

For nearly all nativity and race groups the highest. . . scores are made by men committed for fraud, and the lowest scores by men committed for sex crimes. The relative order of the other crime groups is less consistent, with robbery usually following fraud, then larceny, burglary, and murder (1939: 155).

Nearly three decades later, West's summary of the research literature suggests no substantial change in the relationship between type of crime and level of intelligence.

There is a demonstrable relationship, . . . , between the average intelligence scores of those who commit different types of crime. As groups, sexual offenders and those guilty of personal violence are on average less intelligent than thieves, and of course considerably less intelligent than embezzlers (1968: 112).

It would appear, then, that there is this basic conclusion to be drawn from our brief discussion: intelligence appears to bear a varying relationship to criminality according to the type of offense considered.

A second focus in the search for psychological correlates of deviant behavior has emphasized the role of personality factors in criminogenesis. Research efforts in this direction have faced many of the same problems encountered in the early attempts to relate intelligence to criminality. In short, a specification of terms was required. Thus McCann (1948) points out that vague conceptualization (e.g., the term "mental illness") has often resulted in linguistic and semantic fallacies that have distorted attempts to relate crime and personality. Dunham makes the same point in noting that attempts to associate crime with "mental disorder" have suffered from the fact that this inclusive label covers a multitude of clinical symptoms and different forms of behavior. Dunham goes on to note that indiscriminate quantification



provides no remedy for such conceptualization. It comes as no surprise, therefore, that "Such studies range all the way from reporting fifty-nine per cent of a sample of criminals as emotionally and mentally defective, to reporting only five per cent so affected (1939: 352).

No more encouraging in its findings is a review by Schuessler and Cressey of 113 attempts to locate differences between criminals and non-criminals on personality tests. Schuessler and Cressey find that only 47 of the tests (42 per cent) succeeded in demonstrating a significant difference between the two groups. The authors conclude that ". . . as often as not the evidence favored the view that personality traits are distributed in the criminal population in about the same way as in the general population" (1950: 483). Vold (1958: 127), however, aptly points out that this survey mixes together in a rather indiscriminate manner a wide range of "well, badly, and indifferently controlled studies." The result is that the percentages computed overall are of questionable validity.

Another direction taken in research attempting to test the linkage of personality factors to crime is incorporated in the Gluecks' Unraveling Juvenile Delinquency (1950). The Gluecks have favored an eclectic approach in their research. On the psychological level, this has involved an attempt to interrelate personality characteristics, as indicated through psychological tests and interviews, with the ultimate goal of developing prediction tables. Such tables were constructed after an in-depth study of 500 delinquent and 500 non-delinquent boys. Wootten (1959: 186) and Vold (1958: 131) indicate that these tables have been used with notable success.<sup>2</sup> Although reviewers have questioned the lack





of a coherent theoretical base supporting these prediction tables, the Gluecks do provide some hints toward an overall pattern that may characterize their findings:

It is. . . the manner in which they [delinquents] typically resolve such conflicts that the distinction between the two groups under comparison weaves most meaningfully into the general pattern. More than twice as many delinquents tend to resolve mental conflicts by extraversion of action and/or feeling. . . while, by way of contrast, eight times as many non-delinquents as delinquents tend to resolve their conflicts by introversion (1950: 275).

Another attempt at prediction of delinquency using psychological tests has involved the Minnesota Multiphasic Personality Inventory (MMPI). Among the more successful efforts in this area are those reported by Monachesi (1950) and Hathaway and Monachesi (1957).<sup>3</sup> Capitalizing on the success of experiments utilizing the MMPI, particularly the "psychopathic deviate scale" of this inventory, Argyle (cited in Glueck, 1965) has attempted to integrate the personality traits predicting criminality into several distinct dimensions. Perhaps the most important of these dimensions is labelled "weak ego control" or "impulsiveness"--a category that corresponds well to the Gluecks' discussion of "extraversion." Thus both the MMPI and the Gluecks' prediction tables seem to provide independent support for at least one personality trait associated with criminality. However, this seems to represent only one segment of a consistent theme running through a great deal of recent research.

Porteus (1959) established another base for research along these lines with an adaptation of his maze test. The Porteus Maze Test was initially conceived as a psychomotor test of intelligence. However, Porteus became aware of the possibility of using his test for the



prediction of delinquency as early as 1915, when he found that a group of 22 delinquent boys in Australia scored more than two years below their chronological age. Jarrett reported similar findings in a study of 100 youths in the English Borstal system in 1926. But it was not until 1942 that Porteus teamed up with Honzik to devise a new system for scoring the qualitative errors made by delinquents in their performance on the maze test (1959: 85).

In a first test of the qualitative scoring system, "Q-scores" were compared for 200 delinquents, 100 criminals, and 100 members of a control group. The results of this test indicated that delinquents made twice the number of errors as did non-delinquents, while criminals made more than three times the errors reported for the control group. Porteus reports that these differences were statistically significant (1959: 87).

During the years that followed this first extensive test of the new scoring system, two additional independent examinations were also carried out using two new samples of delinquents. The first of these tests was conducted by Wright in 1944, using Q-scores from a group of 54 delinquent boys. Results of this test replicated nearly exactly those reported by Porteus. The mean error score of the delinquents in this sample was 49, as compared to a score of 22 for the original non-delinquent control group supplied in the Porteus study. A second study was carried out by Grajales on 60 delinquents in New York City. The mean delinquent score in this experiment was 56, representing an increase over the error scores found among delinquents in earlier tests (Docter and Winder, 1954).

These studies were followed by still another application of the





Maze test by Porteus to several new delinquent and non-delinquent groups. This time the non-delinquent mean scored dropped and the delinquent score increased, thus widening the gap between non-delinquent and delinquent average performances, and thus increasing the significance of the difference (Porteus, 1959: 90).

The most careful of the studies attempting to replicate the original findings of Porteus was carried out by Docter and Winder (1954). In contrast to the earlier studies by Wright and Grajales, this research effort utilized both an experimental group (delinquents) and a control group (non-delinquents). The two groups consisted of boys matched for mental ability, race, and socio-economic level. The results of this study indicate a mean Q-score for the experimental group of 47 and a mean Q-score of 25 for the control group. The authors conclude that "The major finding, then, is that the qualitative performance of delinquents and non-delinquents corresponds almost exactly with the results previously reported for such groups, and that the difference between the delinquents' and non-delinquents' means. . . is significant at the .0001 level" (1954: 71-72).

The consistency of the performance of delinquent and criminal groups on the Porteus Maze Test and the power of the derived scores as instruments in the prediction of deviant behavior are quite striking. But again, as with the Gluecks' predictive tables and the Minnesota Multiphasic Personality Inventory, there is a puzzling lack of theoretical rationale offered in explanation of the test's success. We know that performance on the maze tests involves psychomotor skills, but little systematic effort has gone into describing the relationship



between the expression of these skills and deviant behavior. Docter and Winder do, however, suggest one theoretical possibility that re-introduces a theme mentioned earlier:

. . . one guess which the writers suggest as reasonable is that this measurement of expressive movements taps one or more personality variables related to the construct of ego-control. Impulsivity may best describe the cluster of responses which contribute to the Q score. Should this be the case, and future research may clarify the matter, instruments of this kind would appear to be an important contribution toward the objective assessment of personality (1954: 73, emphasis added).

A wide range of related types of research efforts are available that deal with the relationship of impulsivity, and similar constructs, to criminality. The first of these studies is particularly significant in that it utilizes a longitudinal research design; Kelley and Veldman (1964) have in this way demonstrated that poor performance on psychomotor tests is not simply a consequence of criminal behavior. Eight hundred and eighty-four male subjects were used in the experiment, and the results were controlled for the influence of social class effects. In the final analysis, the deviant groups differed significantly from the non-deviant group on three psychomotor tests and on two tasks that required the maintenance of a convergent set and surgency. The authors conclude that their findings are consistent with the "impulsivity postulate" (1968: 190).

A second study by Lowe (1966) deals with the problem of impulsivity under the title of "response inhibition." The experimental subjects in this study were 157 educationally subnormal children, ranging from 7 to 12 years of age. The experimental situation used in this study was, to say the least, provocative:





The child under testing sat at a table, holding in his preferred hand a toy, sawn-off Luger pistol with a sensitized trigger which was connected to the recording unit. He was instructed to press once as soon as he saw the yellow light, but not to press when the green light was shown. With auditory stimuli he was told to press when he heard the buzzer, but to refrain from responding to the bell (1966: 926).

Correlated with the subjects' response errors in the experimental situation were their behavior ratings, as assigned by teachers using Stott's Bristol Social Adjustment Guide. Results of the experiment indicate that in this subnormal experimental group, error score and behavioral rating are significantly correlated. Lowe concludes that impulsivity, or inadequate "response inhibition," may well be the psychological precursor of deviant behavior in adults.

Probably the best known scheme incorporating the notion of impulsiveness is that proposed by Eysenck in his discussion of extraversion and neuroticism as personality traits associated with criminality. The attempt here has been to link extraversion and neuroticism with the incidence of criminality through the intervening variable of "impulsiveness" or "conditionability." Eysenck (1964) begins his work with an extensive review of theoretical and empirical attempts to outline the various types of human personality. After reviewing the work of Galen, Kant, Wundt and C. G. Jung, Eysenck suggests a conceptual summarization utilizing two major dimensions of personality: the first of these refers to an axis reaching from neuroticism to stability, while the second refers to an axis ranging from extraversion to introversion (1964: 33-36).

The foundation of Eysenck's theoretical model, then, rests on the two dimensions of personality just described. In turn, it is postulated



that these dimensions of personality have a physiological basis that is genetically transmitted. Environmental effects are superimposed on these biological givens--either mitigating or aggravating the situation. The emergent behavioral disposition is characterized in terms of conditionability. After weighing the evidence both pro and con the conditionability postulate, Eysenck feels safe in concluding that "In spite of all the difficulties, the evidence still suggests that conditionability as a general substrate of behaviour, is a meaningful concept, and may be retained with some advantage" (1964: 85-86).<sup>4</sup>

Given this preliminary (and guarded) empirical endorsement of Eysenck's theoretical model, we must next move on to the larger question of how validly individuals can be located on the two dimensions of personality proposed. Operational definitions of the polar types of personality characterizing these dimensions are, of course, necessary before this question can be answered. One operational definition used to place individuals on the neuroticism-stability dimension suggests that the former condition describes people one would "expect to break down fairly easily given some degrees of stress," while the latter are those people in whom "only the very greatest stress would produce neurotic symptoms" (1963: 52). An operational definition used to place individuals on the extraversion-introversion dimension, on the other hand, focusses on the familiar concept of impulsiveness. Extraverts are, very generally, impulsive individuals, while introverts are typified by a distrust of the "impulse of the moment" (1964: 35-36). These operational definitions are intentionally impressionistic in tone, for they are to be used in connection with scales of "self-ratings" and "ratings





by others." The validity of locating individuals on Eysenck's two axes by means of the two methods described can be tested by intercorrelating the respective "self" and "other" scale scores.

Eysenck has carried out such a test and has concluded that placement of individuals on the extraversion-introversion scale can be accomplished with assured validity, but that the validity of placement on the neuroticism-stability scale is of a more tentative nature.

. . . the results of this experiment suggest unambiguously that as far as extraversion is concerned, self-ratings and behavior as rated by others agree well; as far as neuroticism is concerned, the picture is rather less clear, but. . . we may regard the self-ratings as valid, and the ratings as rather less so. This conclusion is [reached by means of] . . . agreement with the theory put forward by S. B. G. Eysenck. . . extraverted behavior is more easily observable by the outsider, whereas neuroticism is more characterized by subjective internal conditions, such as anxiety and other conditions, which may not give rise to observable differences in behavior (1963: 56).

If we accept the validity of the dimensions proposed by Eysenck for the differentiation of human personalities, and there seems sufficient reason to do so, then our next task is to explore their relationship to criminality.

Eysenck has worked towards this second task by the use of the Maudsley Personality Inventory, a 48-question, self-rating instrument designed to measure the two dimensions of neuroticism-stability and extraversion-introversion. Eysenck notes that this personality inventory measures two independent variables and that the results have been successfully examined for reliability and construct validity. The inventory has been applied to a variety of groups and the results have been summarized so as to demonstrate differentiation between normals and neurotics and, most importantly for our purposes, between the previously mentioned



groups, criminal recidivists and psychopathic groups (1959: 176-77).

The results of various studies summarized and reported by Eysenck in 1959 indicate that group variation from the origin of the neuroticism-stability axis increases for neurotic, criminal recidivistic, and finally, psychopathic sample groups. More specifically, psychopaths show the most neurotic scores on the neuroticism-stability scale, followed by the criminal recidivists, with the neurotic group scoring less than either of the previous groups, and a final normal or control group scoring lowest in terms of neuroticism. On the extraversion-introversion axis, the psychopathic group again shows the most variation, followed, however, by the neurotic group and normals, with the criminal recidivists demonstrating the least extraversion (1959: 177). A more recent summarization of findings by Eysenck (1964) generally repeats the 1959 results, but with one extremely important change: the placement of criminal groups on the extraversion-introversion axis. In this summary, criminal groups show scores extremely close to psychopathic groups on both neuroticism and extraversion scales. This last set of findings is, of course, more in accord with Eysenck's original hypothesis. In this regard, it should be noted that the former collection of findings utilized only one criminal group, while the latter study included a large sample of criminal groups (drawn from several countries). It would seem to be the latter study, therefore, that offers the more complete review of the current research literature.

Eysenck is most impressed with his findings in relation of psychopathy. He acknowledges that it is a critical error to identify psychopathy with criminality; with certainty, a person may be psychopathic





and still not be institutionalized or guilty of illegal acts. But, on the other hand, Eysenck also argues that psychopaths should be expected to contribute more than their share to the delinquent and criminal population. Eysenck concludes:

. . . the psychopath presents the riddle of delinquency in a particularly pure form, and if we could solve this riddle in relation to the psychopath, we might have a very powerful weapon to use on the problem of delinquency in general (1964: 41).

Whether or not we are disposed to agree with Eysenck's interpretations of his findings, it seems that we must acknowledge the findings themselves. And according to Eysenck's most recent summary of research relevant to his hypotheses, criminal populations are both more neurotic and more extraverted than "normal" segments of society.

Another recent attempt to associate personality traits with deviant behavior is presented in the work of D. H. Stott (1968). Stott's research is important for at least three reasons. First, he presents a new theoretical construct--inconsequence--that brings together a number of the personality characteristics that we have considered in the previous pages. The following passage illustrates this point well:

The primary characteristic of the inconsequential child is that he is dominated by the stimulus of the moment. . . . What is impaired in him is his capacity for temporal integration: there is no past and no future, but only the present. . . . Because of his lack of temporal integration he never learns wisdom from his previous follies; in psychological terms he is resistant to conditioning (1968: 19, emphasis added).

A second important aspect of Stott's work involves his development of the British Social Adjustment Guides, an instrument designed to detect and diagnose inconsequence, as well as other forms of maladjustment. Use of this instrument provides a new independent measure of several



aspects of personality involved in criminality. Finally, Stott's research merits close attention because he is currently in the midst of testing the utility of the Adjustment Guide as an instrument of delinquency prediction. As distinct from most earlier efforts to assess the merits of prediction devices, Stott is using what he refers to as an "unselected boy population" as his experimental sample. Most other tests of predictive efficiency have been biased through the use of "artificial populations" in which delinquents were many times over-represented (1968: 64).

A final study conducted by West (1969), using the same type of predictive test recommended in Stott's work, is also currently being carried out in England. This research effort promises to be perhaps the most ambitious project to date. The sample in this study consists of 411 boys recruited during the fourth year of school, from a densely populated, working class, urban district. The project, now in its second phase, incorporates a wide range of psychological tests tapping, among other things, the dimension of impulsivity discussed in the previous pages. Among those instruments being used are Eysenck's Junior Maudsley Personality Inventory, the Porteus Mazes, the Gibson Spiral Maze and the Body Sway test. The long-term results of this study thus promise to provide some rather definitive indications of the relationship between independently measured traits of personality and deviant behavior. The authors regard the findings to this point as being very promising in terms of confirming the predictive value of the instruments described above. The most useful of the psychological tests appears at this stage to have been those devices aimed at measuring psychomotor skills.





Our review of the research literature surrounding attempts to relate psychological differentiae to criminal behavior has yielded some rather interesting results. We feel safe in recommending the tentative conclusion--subject to revaluation in light of continuing research efforts of the type being completed by Stott and West--that psychological differentiae do exist in a manner relevant to criminal behavior. More specifically, we have catalogued a considerable amount of research indicating that the interrelated concepts of extraversion, impulsiveness, response inhibition, conditionability, neuroticism and inconsequence are significantly associated with criminal behavior. To a lesser extent, we have also accumulated evidence indicating psychological differences with respect to intelligence between various types of criminal and non-criminal populations.

These findings would seem to suggest that the neo-Chicagoans have been remiss in neglecting some of the psychological aspects of criminogenesis. Our evidence thus recommends rejection of the first assumption of the interactionist perspective in deviance.

Hypothesis II: "Taking the Deviant's Viewpoint" Is an Effective Means of Gaining an "Understanding" of his Behavior

The neo-Chicagoans have argued that by "taking the viewpoint" of the deviant, they are able to "understand" his behavior. In Chapter One, we noted the difficulty of knowing what the interactionists mean by the phrase "taking the viewpoint" as well as the problem of clarifying what is intended in their use of the term "understanding." However, it was also noted that researchers in the field of person perception have commonly used judge's predictions of target other's responses as an



indicator of "role-taking" activities, while additionally using the correspondence of these predictions with actual responses as an indicator of the accuracy of the "knowledge" or "understanding" gained through these procedures. The latter set of operational definitions have been adopted as a means of evaluating the neo-Chicagoans' "role-taking" hypothesis.

A major concern in studies utilizing the research design described has been the role played by similarity, real and assumed, between the understanding judge and the target other. One aspect of engaging in empathetic procedures may involve, although it need not, the projection of one's own cognitive set onto the target. This tendency to engage in assumptions of similarity is productive of a fortuitous bias when the similarity between the judge and target is real, thus confusing attempts to measure the "ability to role-take." On the other hand, when no such similarity exists, the tendency of the empathetic role-taker to project may be productive of errors of "assimilation." Experimental studies indicating the sources and implications of assimilation errors are available.

For example, Warr and Knapper have accumulated considerable empirical support for the instructive hypothesis that "persons who are liked are perceived to be similar to oneself" (1968: 250). Given the "appreciative" attitude shared by the interactionists for the deviant underdog (cf. Matza, 1959: 15-17; Gouldner, 1968: 106), this postulate takes on considerable relevance to our discussion. Berkowitz and Goranson (1964) have demonstrated, in accord with Warr and Knapper's hypothesis, that sorority girls in an induced liking situation minimize objective





differences between themselves and others. Backman and Secord (1962) have reinforced the liking-assumed similarity postulate with parallel findings in another study of sorority girls. The instructions gained from such experiments are, again, of relevance when considered in view of the interactionists' admitted affection for the deviant.

Additional findings from the Backman-Secord study indicate that the more a subject interacts with other, the more he will distort his presumptions of other's perceptions in accord with his own perceptions. Thus the assumption of similarity apparently varies directly with the amount of interaction with other. This fact is cause for concern when we recall the neo-Chicagoans' argument that we must become fully involved in the world of our deviant subjects in order that we may more accurately "understand" them (cf. Blumer, 1969: 37).

Further evidence bearing on the dangers of assumed similarity comes in the form of Kelly's (1949) findings regarding the effects of expectations upon our first impressions of others. It appears that, at least among 65 third-year college men, artificially induced expectations of a "warm" or "cold" instructor yield first impressions in congruence with differentially manipulated preconceptions. The implication is, of course, that initial assumptions of similarity may also be paid back in kind with reinforcing first impressions. The persistence of these impressions over time is as yet formally untested; however, Backman and Secord's study, noted above, would seem to suggest that such impressions would be intensified with continued interactions.

Additional support for the perceptual bias we have associated with preconceived assumptions of similarity is provided in Luchin's (1957)



experiments involving primacy-recency effects in impression formation. Luchin has been concerned with evaluating the differential effect of information presented to experimental subjects in various sequences. His experiments have involved presenting subjects with inconsistent communications and assessing their relative impact for indications of primacy or recency effects. Luchin concluded after the first of his experiments that "For all. . . groups studied, each of the. . . indices pointed to primacy" (1957: 39). After varying his experimental situation in a number of ways, and after analyzing post-experimental interviews with his subjects, Luchin indicates that ". . . qualitative results corroborate the quantitative data by suggesting that the characteristic reaction to inconsistencies, when they were noticed, was to pay greater heed to earlier behavior" (1957: 54). The implication of this research effort would seem to be that initial assumptions of similarity will be stubborn and resistant to change, regardless of contradictions to these assumptions by newly encountered facts.

The conclusion to be reached, then, is that the tendency to project or assume similarity may be productive of errors of assimilation. This does not mean, however, that (1) real similarity is the only condition under which accurate predictions can be made or (2) that the assumption of similarity is productive of completely inaccurate predictions. In fact, Blanchard (1967) reports an experimental study in which the perception of similarity between the judge and target is controlled. The findings of this study reveal that when information relevant to the prediction of other's response is supplied, the judge is more accurate than by chance on both similar and dissimilar targets. As noted in





Chapter One, however, the real question to be answered is this: Is the procedure of "role-taking" the most effective means of "knowing" about other's behavior? The question posed becomes answerable if it is assumed that a measure of "knowing" is the ability to make accurate predictions concerning the subject(s) under study. Acting on the basis of this assumption, the relative utility of "role-taking" can be gauged by comparing the predictive success of the clinician's use of empathetic methods with the actuary's use of statistical tables.

Clinical psychologists and psychiatrists, like the interactionists, have often advocated and practiced the use of empathetic methods, in preference to actuarial techniques, for the prediction of delinquency, crime and recidivism. A comparison of the predictive accuracy of clinical versus actuarial techniques thus conveniently juxtaposes a method assuming the utility of "role-taking" with a procedure that avoids this assumption. The results of such comparisons are, consequently, a measure of the relative utility of "taking the other's viewpoint." Comparisons of this type have been recorded in the area of deviant behavior by Meehl and Wootton.

Meehl (1954) has compiled a comprehensive review of twenty studies, three of which focus on criminal behavior, involving empirical comparisons of clinical and actuarial techniques. The first of the three studies dealing with criminality is a research effort completed by Burgess in 1928. Burgess investigated the outcome of 1,000 parole cases in three Illinois state prisons. The predictive instrument built from this analysis utilized 21 objective measures (e.g., crime, length of sentence, etc.) to be used in a simple count of factors operating for or against



successful parole adjustment. The predictions calculated by this method were then compared to those formulated by two prison psychiatrists.

When the predictions involved a forecast of successful parole adjustment, both clinicians performed slightly better than the actuary--the former netting 80 and 85 per cent accuracy and the latter scoring 76 per cent accuracy. However, when predicting failure in parole adjustment, both psychiatrists finished far behind the statistician--30 percent and 51 per cent accuracy versus 69 per cent accuracy. On the basis of this and other factors in the Burgess study (e.g., the psychiatrists did not predict outcomes in all possible cases), Meehl scores this comparison in favor of the actuarial method (1954: 95-96).

A second study by Borden, also completed in 1928, again compared predictions of parole violation, this time for 261 ex-reformatory inmates. The three predictors--previous record, intellectual level, and "psychological prognosis" (the clinical prediction)--were compared for relative success in forecasting parole adjustment. The findings reveal that "previous commitments" yielded a very low correlation with parole outcome, but that the other two predictors scored even lower. However, further analysis reveals that the optimal combination of both actuarial factors (intellectual level and previous commitments) far exceeds the efficiency of the clinical prediction by the psychologist. And again, it should be noted that the psychologist balked at predicting parole outcomes in all cases. Although neither the clinical nor actuarial method shows up impressively in this study, Meehl concludes that the research can presumably be counted in favor of the actuarial method (1954: 103-04).

Another comparative study cited by Meehl is Hamlin's 1934 analysis





of institutional adjustment among 501 reformatory inmates. Here the predictive test involved estimation of adjustment over a four- to ten-month period. Selecting from a list of over 100 predictive items, 15 "non-overlapping" items were separated out providing a predictive score. Meehl goes on to note that eleven or twelve of these items represent "purely objective factors." He concludes that "Although the author does not compute a multiple R based on the eleven or twelve purely objective factors alone, inspection of the table, . . ., surely justifies us in saying that the actuarial prediction would be at least as efficient as any of the clinical or administrative estimates, and very probably more efficient" (1954: 105).

Wootton (1959) has reviewed several more recent empirical comparisons of empathetic and actuarial attempts at prediction. The first study considered is the Mannheim-Wilkins enquiry into the after-history of juveniles detained in the British Borstals. The predictive tables used in this study were, as is usually the case, prepared by authors who had no experience with the subjects in question. Forecasts based on these tables were next compared with predictions made by clinical psychologists who had extensive experience with the juveniles whose careers they were considering. The results again reveal that statistical procedures were significantly more accurate than the psychologists' prognoses (Wootton, 1959: 184-85).

The most convincing of the empirical comparisons between the two methods, however, is to be found in connection with the Cambridge-Somerville Youth Study. The importance of comparing the application of actuarial and empathetic techniques in this setting is that the sample



provided by the Cambridge-Somerville project was independent of the population from which the actuarial table was originally derived. The statistical scale used was in fact developed years in advance by Sheldon and Eleanor Glueck. Eleanor Glueck applied a version of the "Glueck Social Prediction Scale" on a sample of 100 boys from the Cambridge-Somerville project, and her forecasts were then judged against the predictions of a selection committee consisting of one psychiatrist and two social caseworkers. The results indicate that the Gluecks' predictions proved correct in 91 per cent of the cases as compared to a score of from 61.5 per cent to 65.3 per cent of correct forecasts by members of the committee (Wootton, 1959: 185-86).

In sum, it appears that both studies reviewed by Wootton, in addition to the three considered by Meehl, all fall into the actuarial success column. Procedures incorporating empathy and presumably based on role-taking are thus shown to disadvantage. It would appear that the benefits of "taking the other's viewpoint" are less than the neo-Chicagoans' arguments would lead us to believe. When the measure of success is predictive accuracy, selective statistical attention to various identifying characteristics of the subjects-to-be-studied appears to be the more rewarding approach.

### Hypothesis III: Behavior is Mediated by Thought

Behavior is, we acknowledged in Chapter One, mediated to some degree by thought.

Having acknowledged this truism, one is immediately faced with the perplexing awareness that the meaning of the term "thought" is extremely vague, and that what empirical knowledge we have regarding its operations





suggests that its influence is of a fluctuating character across time and space. Four interrelated questions, therefore, remain to be answered: (1) what varieties of behavior are mediated, (2) in what situations, by (3) how much thought, where (4) behavior, thought, and the mediation process are identified by which criteria?

Our argument has been that answers to these questions must be preceded by the specification of independent criteria for the measurement of thought, behavior and the process(es) mediating the relationship between thought and behavior.

Efforts to satisfy this requirement and to construct a general theory or perspective, such as symbolic interactionism, relating thought to behavior have floundered in attempts to measure the more elusive of these variables--thought. Sociologists have preferred to measure thought through verbalizations.<sup>5</sup> Unfortunately, as will be discussed in a later section of this chapter, this procedure has been short on encouraging results. Although we have developed exceedingly sophisticated techniques for measuring attitudes, as expressed verbally by our subjects, we still remain on shaky ground when our methods are summoned for their ostensible purpose of predicting behavior. An additional difficulty consists of the fact that verbalizations have often been accepted as indication of both thought and behavior, rendering intended propositions into the form of apparent tautologies. All things considered, we would probably benefit from an alternative choice in criteria for the measurement of thought.

Physiological measures probably promise the best long-term results as a criterion of thought. Certainly the most sophisticated and advanced



investigations into the mechanisms of thought are being conducted in the fields of psychobiology and neuropsychology. However, even in these rapidly growing fields, our knowledge is as yet insufficient to allow other than the most general predictions, and our physiological indicators of thought are correspondingly imprecise. For example, interference, manipulation and rearrangement in the structure of the brain in both humans and animals have given us a considerable amount of information with reference to functions of various parts of the "physical mind." We also possess some knowledge about the manner in which communication occurs between the cerebral hemispheres; communication of this type seems to occur in a "brain code." Evidence indicates that this code consists of nerve impulses and excitatory effects in nerve cells and fibers and perhaps also in the glia cells of the brain. Further indications suggest that this brain code is built of spatio-temporal patterns of excitation. But as Sperry (1965) concludes, this preliminary information still leaves us quite far from being able even to imagine the critical variables in the brain code that might correlate with the variables that the mentalists are fond of discussing in their descriptions of cognitive experience.

The facts simply do not go far enough to provide the answer, or even to come close. Those centermost processes of the brain with which consciousness is presumably associated are simply not understood. . . we are still hopelessly lost (Sperry, 1965: 76-77).

Thus, while "brain research" continues to be a productive undertaking with exciting long-term implications, it still remains short on those types of specific indicators needed for the construction of a generalized theory relating thought to behavior.

Despite these difficulties, however, Sperry (1965; 1970) has





aligned himself with what he regards as a miniscule "mentalist minority" among brain researchers, who condone the use of mentalist concepts in lieu of further physiological knowledge of the mind. And, like the symbolic interactionists, Sperry soon finds himself travelling in the circles of redundancy characteristic of discussions involving undefined concepts. Perhaps by explicating the futility of Sperry's exercise we can demonstrate, in the neutral field of psychobiology, the disservice a redundant mentalism does to attempts to know the relationship of thought to behavior. Our discussion will be based on Bindra's (1970) excellent critique of Sperry's recent work.

The major part of Sperry's "theory" revolves around the proposition that conscious awareness has a causal influence on the sequence of neural events that produce behavior. This proposition is extended with two hypotheses: the first postulating the neural basis of awareness; and the second postulating the role of consciousness in determining neural activity and behavior.

The first of these hypotheses states that certain interactions of sensory inflow and ongoing central nervous system activity generate an emergent higher order organization of neural excitation with the property of consciousness. Bindra notes that this hypothesis implies higher order or "conscious" organizations that are distinct from lower order or "non-conscious" organizations. But the characteristics that would distinguish these two levels of organization are not specified. Instead, Sperry leaves the reader to travel in the circular knowledge that conscious awareness is the outcome of processes that generate conscious awareness, and that these constitute 'higher order' cerebral organizations.



In effect, then, Sperry has proposed no hypothesis about the neural mechanism of conscious awareness. A genuine hypothesis would characterize the 'higher order' organizations in substantive neural terms independently of conscious awareness (Bindra, 1970: 582).

Sperry's second hypothesis states that conscious awareness (accompanied by "higher" order cerebral organizations) is in a position of "higher command" and can govern the flow of nerve impulses through its encompassing emergent properties. But again, this hypothesis supplies no indication of the type of differences in the cerebral organization which determine the presence of the controlling influence of a brain locus. We are supplied no means for independently identifying the existence of higher order, emergent, or conscious influence.

Thus, his assertion that subjective experience causally influences neural activity rests on nothing more than a semantic equating of conscious awareness with 'higher order' cerebral organization and with 'higher command' or governing influences of certain neural organizations on others. As such, the assertion remains a tautology, and we are no closer to having a hypothesis regarding how subjective experience influences neural actions, nor having a proof that it does (Bindra, 1970: 583).

Sperry (1970), for his part, complains that he has been misinterpreted. He expands on his position by noting that holistic interaction is presumed to apply to cerebral activity, both at conscious and non-conscious levels. Further, it is noted that higher order activities are also both conscious and non-conscious, although consciousness does necessitate higher order cerebral activity. Thus,

It is not mere complexity or high order organization that in this scheme endows a neural event with conscious awareness. It is rather the specific operational design of the cerebral mechanism for the particular conscious function involved (Sperry, 1970: 589).

It is difficult to understand, however, how Sperry's "clarification"





saves his formulation from either vacuity or redundancy. And perhaps even Sperry, in the end, is willing to come close to this conclusion:

Bindra is quite correct in stating that I have not defined in concrete terms the exact organizational features of the neural process responsible for the conscious effects, nor advanced a definitive proof that subjective experience influences neural actions (Sperry, 1970: 589).

What this long-winded discussion ends up saying, then, is that we simply are not in a position to add much detail, from a psychobiological view, to the uninformative truism reaffirmed ad infinitum by the mentalists: that behavior is mediated, to some degree, by thought. The reintroduction of mentalist concepts, whether the field be psychobiology or sociology, merely fills this vacuum with redundancy.

Thus it seems that we must repeat a point made earlier: there is no current basis for formulating a generalized scientific theory relating thought to behavior. The basis for such a theory may require a more detailed neuro-physiological knowledge of how the brain operates than is now possessed. The social scientist is thus encouraged to assume an attitude of humility in approaching the thought-behavior problem; part of this humility suggests that attention be focussed on those aspects of the thought-behavior problem that are amenable to statement in the form of specific hypotheses.

#### Hypothesis IV: Perceptual Discrimination is Regulated By the Manipulation of Linguistic Symbols

In Chapter One, we noted that the terms used in the neo-Chicagoans' discussions of the thought process (i.e., "interpretation," "meaning" and "definition") suggest an intended reference to the ability to make perceptual discriminations. That is to say that "interpretations" and



"definitions" can only be recognized in perceptual discriminations.

This ability to make perceptual discriminations is assumed to depend upon the capacity to manipulate corresponding verbal symbols. An equation is thus postulated between the act of discrimination and the manipulation of "language-able" symbols.

The relationship postulated by the interactionists between the act of perceptual discrimination and the process of verbal symbolization seems productive of a systematic bias. Such a bias places a heavy burden on elements of "consciousness" and "rationality." If we operationalize consciousness as the capacity for verbalized awareness, this bias can be subjected to empirical investigation. An investigation of this type is presented in the following pages with the goal of gaining insight into "how much" perceptual discrimination is regulated by the conscious manipulation of linguistic symbols.

An exhaustive survey of research in the area of non-conscious behavior has been provided by Eriksen (1960). There is a dual focus to this review of the literature. Attention is first paid to subliminal discrimination--a phenomenon characterized by differential responses to stimuli whose intensity is below that where a differentiated verbal report can be obtained. The second focus is on incidental discrimination--a phenomenon involving differential responses to stimuli above threshold, but where the subject is still unable to identify the stimuli used for his responses.

Among the experiments dealing with subliminal discrimination, Eriksen seems most impressed with the work of Lazarus and McCleary. In this experiment, galvanic skin responses (GSRs) were conditioned to





5 of 10 nonsense syllables. These nonsense syllables were then projected on a tachistoscope for the experimental subjects. Findings revealed that even on those trials where subjects were unable to provide correct verbal identification of syllables that were accompanied by shocks, the average GSR to shock syllables was still greater than to non-shock syllables. The authors thus interpreted their results in terms of "subception"--a phenomenon whereby some kind of discrimination is made, even though the subject is unable to make a correct verbal discrimination.

In general, however, Eriksen is less than overwhelmed by most of the experiments dealing with subliminal discrimination. More convincing, it is argued, are those experimental findings dealing with incidental discrimination. Thus Eriksen prefaces his consideration of experiments in this category with the following remarks:

Common sense tells us that we are constantly utilizing cues of which we are unaware. . . . Experiments appear to have demonstrated not only the use of cues without awareness but also the lack of awareness in some instances for the response the S is making, for the reinforcements controlling the response and in some cases the contingent relationship between cues, responses, and reinforcements (1960: 293).

The experiments to which Eriksen refers involve what he prefers to call "performance without awareness." Involved here are several experiments demonstrating that unnoticed cues are capable of producing perceptual illusions. The best known of these demonstrations include: (1) Bessler's experiments using the Müller-Lyer Illusion, where the directional arrows on two sample lines were so faint the subjects reported that they were unaware of their presence; (2) Miller and Perky's use of geometric forms projected on the back of a ground glass screen, such that experimental subjects reported accurately the presence of the geometric



forms, without awareness of the low intensity projection; and finally (3) various investigators' (e.g., Smith and Henrikson) use of the meta-contrast phenomenon as a method of presenting cues in such a way that subjects were unaware of their presence. Perhaps also we should mention in this category the experiments of Poetzl, who exposed his subjects to complex pictures tachistoscopically and instructed them to dream about the details. Poetzl later found that details of the pictures which were not reported in "intentional recall" tended to be rediscovered in the subjects' dreams.

The implications of the findings summarized by Eriksen is that the role accorded in interactionist explanations to the conscious manipulation of verbal symbols is excessive, when the phenomenon-to-be-explained is perceptual discrimination.

There is nothing profound in noting that language and words are highly abstract symbols and bear little or no physical resemblance to the objects, events, and relationships that they denote. A definition of awareness in terms of verbalization places a heavy burden upon the adequacy of language to reflect the richness of perceptual experiences and images (Eriksen, 1960: 280).

A similar point is made by Lenneberg (1967) in his experiments involving the perceptual discrimination of colors. Using a range of colors as their visual stimulus, Lenneberg and others have, by various methods, "mapped" the "color experience" of numerous languages. A first method employed by Brown and Lenneberg is to start with color words used in a specific language--derived by asking native speakers to write down all the words for colors they can think of. Subjects are then presented with a comprehensive color chart and asked to assign these words to a color region on the chart. The result of this procedure is inevitably





a residue of physical colors to which the subjects assign no names. These color spaces are referred to as "innominate" (i.e., unnamed) regions (Lenneberg, 1967: 339-40).

However, when a second method is used, "innominate regions" of the color space apparently all but disappear. This second method involves showing each color to the sample subjects and asking them to describe in one word or more the referent color. That every color can be described using this method is taken as an indication that although "name-determinacy" for color space may vary by language, regardless, ". . . man, everywhere, can make reference to colors and communicate about color fairly efficiently" (Lenneberg, 1967: 340-43).

Confirmation for this conclusion has been provided on an inter-linguistic level by Lantz and Stefflre (cited in Lenneberg, 1967). Here a third method is used to map the color space of a language. Subject A is isolated in a situation where he has to communicate to Subject B, through verbal description, a referent color which is then to be identified by Subject B in an array of colors. The accuracy with which this task can be accomplished is taken as a measure of "communication accuracy." This measure of "communication accuracy" has been compared by Lantz and Stefflre to a measure of "codability" (a term which Lenneberg equates, for the sake of simplicity, with what was earlier referred to as name-determinacy) as predictors of color recognition. The results were as follows:

When communication accuracy is determined for every color in a specific stimulus array it predicts recognition of that color quite well. . . . Codability, on the other hand, predicts recognizability only in special contexts. . . . From this we may infer that subjects make use of the



ready-made reference facilities offered them through their vocabulary, only under certain circumstances (Lenneberg, 1967: 354).

The implication is clear that language is a flexible servant in the process of communicating percepts, rather than an arbitrary master operating to regulate these percepts.

Lenneberg has gone one step further in driving home his conclusions by duplicating his color recognition experiments on congenitally deaf adults and children. Until the age of six, deaf children demonstrate no developed understanding of any form of language. Thus a comparison of their performance on color recognition tasks with that of hearing groups should be instructive. Lantz and Lenneberg have found in such experiments that deaf children and adults score similarly to their normal counterparts, with only two significant exceptions. These exceptions occur in the area of the blue-green and purple-red color regions. It is important to note that semantic comparisons of color terminologies across languages also reveal marked differences in these color regions. Lenneberg thus concludes:

These experiments show that the semantic structure of a language may influence cognitive structuration where our physiological equipment allows for a range of alternative solutions. But there is no indication that the basic organizational capacities are crippled through a crippling of language proficiency (1967: 362).

Our interpretation of the experiments described in this section suggests that verbal symbolization plays a varying role in the process of perceptual discrimination. The empirical evidence reviewed indicates that conscious manipulation of linguistic symbols is sometimes absent, frequently in accompaniment, and only occasionally a regulator of the discriminatory process. No attempt is made to generalize from the





current findings to other stimulus situations. Nevertheless, the results are sufficient to indicate that the assumption of verbal regulation for perceptual discrimination often confounds observed correlation with presumed causation. The answer to the question of "how much" correlation between verbalization and discrimination is reflective of causation remains a matter for empirical resolution. Our conclusion is simply that the amount of causal regulation seems less than the interactionist bias has led us to believe.<sup>6</sup>

Hypothesis V: An Actor's Verbalizations, as Indicators  
Of Thought, Explain his Behavior

We have noted in Chapter One that the interactionists rely heavily on verbalizations--as indicators of thought--in constructing their explanations of behavior. Taking as the interactionists' meaning of "explanation" that (1) their attention to verbalizations allows them to isolate causes, and/or (2) make better predictions with reference to other measures of behavior, we have rendered their assumption testable. This test requires, quite simply, that we assess the correspondence between kinds of utterances and non-verbal behaviors. Fortunately, investigators in the area of attitude research have been diligent in carrying out just these sorts of comparative studies.<sup>7</sup>

The classic research effort in this area is Richard LaPiere's (1934) study of the discrepancy between attitudes and actions in the field of race relations. LaPiere based the first phase of his research on his own personal observations of a Chinese couple's attempts to obtain food and lodging in their travels throughout the United States. In the final analysis, LaPiere found that only one out of



251 establishments refused their services to the visiting couple.

LaPiere next compared these behavioral findings with a questionnaire inquiry carried out six months after the couple's initial visit. Of the 128 establishments who completed and returned the questionnaire, 92 per cent indicated that they would not offer their services to members of the Chinese race. The immediate implication was that there existed no simple relationship between verbalizations and behaviors.

LaPiere's initial empirical challenge to the basic assumption of attitude research was soon followed up in other behavioral situations with increasingly sophisticated techniques and equally startling results. For example, Corey (1937) carried the attitudes-versus-actions question into the classroom situation. Here the effort involved a comparison of scores made on an attitude questionnaire with the actual cheating that went on in a testing situation. But the unique aspect of this study was that Corey devised a scheme that allowed him to determine the quantitative degree of his subjects' cheating behavior: the amount students changed their test papers when allowed to do their own grading. The resulting scores were then correlated with attitudinal scores derived from a questionnaire measuring verbal opinions toward cheating on examinations. Among those students who actually did cheat, the coefficient of correlation was .014. Corey concludes that "In other words, the attitude questionnaire used--a highly reliable one--gave no hint as to how students would behave. Verbal opinions regarding cheating on examinations were unrelated to actual cheating practices" (1937: 278).

The decades following the appearance of LaPiere's and Corey's pioneering studies brought little in the way of comfort for those who





sought to draw a uniform picture of the relationship between thought, verbalization and action. In field after field, researchers have struggled with the awareness that people behave in contradiction to their verbal statements. Some examples will help to illustrate this point:

Demography: All over the world there exists a discrepancy between expressions of desired, idealized, expected and actual family size (Mauldin, 1965; Berelson, et al., 1969; Hauser, 1967).

Criminal Justice: Findings reveal that jurors with verbalized "scruples" against capital punishment may still impose the death penalty (Zeisel, 1968).

Marketing Research: A study of Canadian consumers indicates that routine purchases are not easily predicted from attitudinal data (Murray, 1969).

Survey Research: Findings reveal amounts of invalidity ranging from a twentieth to nearly a half of the responses received on various types of factual questions (Parry and Crossley, 1950).

Commerce: There is no consistent confirmation for the assumption that verbalized attitude toward one's job is directly related to either job performance or job absence (Vroom, 1962; 1964).

Child Psychology: There is evidence that verbal data (much obtained from the child's mother!) may have serious limitations, particularly when employed to test specific hypotheses (Coopersmith, 1968).



Obviously, the slippage between 'words' and 'deeds' is gaining common recognition among the disciplines.

The study of this slippage is most interesting when applied in the "significant" phases of moral conduct. Much has been made, for example, of the divergence between what people publicly say about others and what they publicly and privately do to them. The field of race relations is a case in point. We have already described the classic study in this area conducted by LaPiere. A later study by Kutner, Wilkens, and Yarrow (1952) used similar research tactics and provided parallel findings. In this case, 11 establishments were visited--all either restaurants or taverns in a single American city--by two white women and one Negro woman. In contra-distinction to the LaPiere study, the white women were, in this case, active participants in the situation. The two white women first entered the establishments and were seated; the Negro woman then followed and joined her white companions. In this situational context, the Negro woman was not denied service in a single restaurant or tavern. As in the LaPiere study, an attitudinal follow-up consisted of a mail survey of the establishments visited, asking if they would accept a reservation for a social group including a Negro member. Receiving no replies, a telephone survey was next carried out. As a result, five of the establishments finally accepted the reservation while six refused. A set of control calls was then made in which the racial composition of the social group was not mentioned; in this context, ten of the establishments confirmed immediate reservations.

An interesting variation on previous research designs in the area of inter-racial relations has been utilized by Defleur and Westie (1958).





The investigators first selected two groups of 23 subjects each from a sample of 250 college students. The subjects were chosen on the basis of their extreme scores on a measure of attitudes towards Negroes and were further matched on a number of other potentially significant variables. The comparison of these two extreme groups was then carried out on the basis of a rather ingenious measure of behavior towards Negroes. A 'standard photograph release statement' was given to each of the subjects asking for authorized permission to use a photograph of the subject sitting with a Negro. The statement contained a list of seven ranked choices ranging from permission to use the photograph for "laboratory use to be seen only by professional sociologists" to "use in a nation-wide publicity campaign advocating racial integration." The overt behavior consisted, then, of the subject indicating the degree of his consent and signing the authorization document. Defleur and Westie report that their findings reveal a small relationship between attitude and behavior towards Negroes.<sup>8</sup>

A slightly different implementation of the research design used by Defleur and Westie is found in a study by Linn (1965). In this instance, the attitude questionnaire closely parallels the behavioral measure used in the previous study: subjects were asked to indicate a hypothetical willingness to be photographed with a Negro member of the opposite sex. Thirty-four subjects who had completed the questionnaire were then chosen to take part in an interview involving a Negro representative of a psychological testing organization. In the interview situation, the subjects were asked to pose for the previously described photograph and to sign "releases" for four ranked uses of the resulting



pictures by the testing organization. In addition, a second Negro was then introduced as a representative of an organization operating a racial integration campaign. Also interested in the pictures, this Negro interviewer supplied three more photographic release agreements to be signed by the subjects. This time there were seven levels of agreement, identical to the seven found on the original attitude questionnaire, and the subjects were again asked to indicate the level of their consent. After comparing this "actual commitment" with the original "hypothetical commitments," Linn found two or more levels of discrepancy in 59 per cent of the sample.

One conclusion that can safely be drawn from the studies of race relations reviewed to this point is that the findings vary both with the instruments used and situational conditions encountered. Further support for this conclusion is found in field studies pursuing the relationship between racial attitudes and residential integration. For example, DeFries and Ford (1968) find a simple and direct relationship between individual attitudes toward Negroes and behaviors in regard to open occupancy, as well as a simple and direct relationship between a person's response to open occupancy and his attitude towards individuals and groups important to him. On the other hand, Lohman and Reitzes (1954) find that members of a labor union with a clear policy of granting Negroes job equality behave in an anti-Negro manner with respect to the movement of Negroes into their neighborhood. The authors conclude that "The majority of the individuals studied were consistent in exhibiting what appeared to be an ambiguous and contradictory pattern in their identification with both the community and the union with regard





to the race relations pattern" (1954: 342).

No less disconcerting is a recent review of current studies in the area of racial attitudes and integration by Pettigrew (1969). Collected in this review is a whole series of survey findings indicating that the last several decades have produced a steady improvement in verbalized attitudes of whites towards Negroes. Despite this verbal harmony, however, Pettigrew finds a quite different picture when he turns to the demographic indicators of inter-racial behavior:

The belief that integration is impossible is based on some harsh facts of urban racial demography. Between 1950 and 1960, the average annual increment of Negro population in the central cities of the United States was 320,000; from 1960 to 1966 the estimated annual growth climbed to 400,000. In the suburbs, however, the average annual growth of the Negro population has declined from 60,000 between 1950 and 1960 to an estimated 33,000 between 1960 and 1966. In other words, it would require about thirteen times the present trend in suburban Negro growth just to maintain the sprawling central city ghettos at their present size. In the nation's largest metropolitan areas, then, the trend is forcefully pushing in the direction of ever-increasing separatism (1969: 62).

Thus, despite the increasing "desirability" of social attitudes towards Negroes in North America, racial isolation continues to grow.

A recent review of studies relating attitudes and behaviors, by Wicker (1969), indicates that the situation with respect to race relations is certainly not atypical or exaggerated. In fact, Wicker concludes, on the basis of the studies reviewed, that,

. . . it is considerably more likely that attitudes will be unrelated or only slightly related to overt behaviors than that attitudes will be closely related to actions. Product-moment correlation coefficients relating the two kinds of responses are rarely above .30, and often are near zero. Only rarely can as much as 10% of the variance in overt behavioral measures be accounted for by attitudinal data (1969: 64-65).



Thus there is apparently very little evidence to support postulations involving a simple or direct relationship between verbalizations and behaviors.

So we find ourselves, nearly four decades since LaPiere first published his alarming findings, still in the midst of a dilemma. And Deutscher (1966; 1969) continually reminds us that North American sociology has done little to theoretically or methodologically resolve this puzzling discrepancy between words and deeds. But there is also caution in Deutscher's criticism; we are advised to avoid the unwarranted conclusion that there exists complete incongruence between what people say and what they do. It is noted that "The empirical evidence can best be summarized as reflecting wide variation in the relationships between attitudes and behaviors" (1966: 247). The conclusion, then, is that the time has come for ordering our knowledge of the relationship between attitudes and actions. Borrowing from C. Wright Mills, Deutscher reminds us of our need to know "how much and in what direction disparities between talk and action will probably go."

Modest overtures towards meeting the Deutscher-Mills' challenge have been made. Ehrlich (1969), for example, has attempted to map out nine social and psychological conditions that intervene between verbalized opinions and behavior. It is, presumably, the interplay of these conditions that determines the degree to which thought, talk, and behavior will find uniformity. A more easily tested formula for forecasting those conditions conducive to the consistency of attitudes and actions is suggested by Tittle and Hill (1970). On the negative side of the ledger, Tittle and Hill hypothesize that ". . . attitude measures should





be least predictive of behaviors occurring in situations which (1) are alien to the subject's customary behavioral context or (2) call for aberrant behavior in a familiar action context" (1970: 152). On the other side of the ledger, however, "Attitude measures should be most predictive of behavior in situations which occur repetitively within the common behavioral context of the individual" (1970: 152-53). In addition to these situational factors, Tittle and Hill also specify several methodological conditions that influence the measured relationship between verbalizations and behaviors. Basically, the prediction here is that studies using multi-item instruments (constructed according to replicable and reliable procedures) to measure both attitudes and behaviors will attain the best results.

Tittle and Hill have tested their hypotheses in a review of the fifteen most frequently cited articles in the attitudes-versus-actions literature. The results are revealing:

Of the four studies that most nearly fulfill the . . . requirements set forth above, three show attitude measures to be highly associated with behavioral patterns. Considering all fifteen studies with no regard for their limitations, six report little relationship, three report moderate . . . relationship, and six report high relationship. . . . The above reconsideration suggests that the degree of correspondence observed is at least a function of (1) the measurement techniques employed, (2) the degree to which the criterion behavior constitutes action within the individuals' common range of experience, and (3) the degree to which the criterion behavior represents a repetitive behavioral configuration (1970: 155).

The implication of Tittle and Hill's findings would seem to be that in the study of deviance, a field that by definition often concerns itself with non-routinized behaviors, we should not expect to find a high level of consistency between verbalized attitudes and concrete



actions. Exclusive attention to the verbalizations of our deviant subjects appears, then, to be both a deceivingly simple and disturbingly inaccurate means of predicting future behaviors. Lacking a criterion better than predictive accuracy for the evaluation of scientific explanations, we conclude that the reliance on verbalizations in the construction of such explanations is a strategy of questionable utility.

Hypothesis VI: "Other's" Definition of Subject  
Determines Subject's Behavior

The "labelling hypothesis," despite its popularity during the 1960's, has not received extensive empirical evaluation. One explanation for the scarcity of such research may be found in the distrust the neo-Chicagoans demonstrate for conventional procedures of hypothesis testing. A less charitable explanation suggests that the labelling thesis may prove to be empirically elusive by virtue of its tendency towards tautology (cf. Chapter One). But regardless of its cause, the problem remains the same: there is little research to choose from in an effort to evaluate the hypothesis under consideration. Thus what follows will, by necessity, be a limited analysis, suggesting only very tentative conclusions.

In proposing that "other's definition of subject determines subject's behavior," the neo-Chicagoans make the implicit assumption that behavioral deviance is primarily, if not entirely, a subjectively defined rather than an objectively distinct phenomenon. Nettler has noted the disadvantage of such an assumption.

Labelling explanations diminish, if they do not neglect, the fact that people do not behave similarly and that the label may correctly identify consistent difference (1969: 9c).





A point of dispute, then, surrounds the question of whether there is more to deviant behavior than the label itself. In a sense, this question has already been answered affirmatively in the discussion of the first hypothesis considered in this chapter. Nevertheless, further evidence on this issue, with specific reference to mental illness, has been summarized by Gove (1970).

Walter Gove begins his evaluation of the "labelling hypothesis" by first focussing on the societal other's definition of mental illness. Using studies by Stor, Nunnally, and Cumming as his data source, Gove concludes that the public lacks accurate knowledge about mental disorders and that the societal image of mental illness as a result is distorted. But the real question, Gove argues, is whether or not the label is applied indiscriminately and without location of distinct behavioral differences.

In answer to this question, Gove reviews seven studies indicating that others resist defining a person's behavior as mental illness until psychiatric symptoms become so severe that they are impossible to deal with in the home or community. In addition, field surveys reveal that respondents only regard descriptions of disturbed behavior as representative of mental illness when the person is presented as dangerous. Thus Gove answers our original question with the following conclusion:

In sum, the evidence strongly suggests that persons, typically, are hospitalized because they have an active psychiatric disorder which is extremely difficult for themselves and/or others to handle. It would appear that the public's stereotype of mental illness does not lead to persons being inappropriately labelled mentally ill. . . . Instead, the evidence suggests that the gross exaggeration of the degree and type of disorder in the stereotype fosters the denial of mental illness, since the disturbed person's behavior does not usually correspond to the stereotype (1970: 877).



Moving beyond the generalized public response to mental illness, Gove next examines the role of mental hospital officials in the labeling process. The question here involves the willingness of hospital officials to ratify the assumption of illness in both voluntary and involuntary commitments. In the case of voluntary commitments, two studies indicate that only approximately 40 per cent of the voluntary applicants are accepted for hospitalization. Involuntary commitments, of course, involve a more complicated process with the police, a court psychiatrist, and a judge often being required. Studies centering on this type of commitment report widely varying results. However, perhaps the most instructive finding reported by Gove is taken from one of the larger studies (including over 1,000 subjects) by Haney and Michielutte. In this study involving psychiatric examinations of persons held for commitment, only 50 per cent of the persons under 65 were found to be mentally incompetent, and only 59 per cent overall were judged incompetent. Other studies focussing on different phases of the involuntary commitment process report higher rates of hospitalization; nevertheless, a number of persons are released at all stages of the screening process. In sum, mental hospital officials do not appear compelled to ratify any or all ascriptions of the "insanity label."

Gove next turns his analysis to the career of the patient once inside the hospital. A basic question here concerns the possibility that the label may outlive the illness--i.e., that the ascription of a label may prevent the passage of the patient back into society after termination of his behavior disorder. Gove acknowledges that "mental hospitals may. . . be debilitating places where patients may come to accept the





preferred role of the insane and may, over time, develop skills and a world view adapted to the institutional setting and gradually lose their roles and even interest in the community" (1970: 880). Yet, this could hardly be the complete story; and with the advent of tranquilizers and the open door policy, Gove argues, many hospitals now offer quite rapid and intensive treatment. In support of this conclusion, Gove cites statistics from the State of Washington emphasizing that the median length of residence in state mental hospitals is only 2.0 months. Further evidence for the transience of "labelling effects" is found in a follow-up study of Angrist, et al., indicating that two-thirds of a sample of ex-patients studied had not been rehospitalized after seven years.

Among those who were rehospitalized in the Angrist study, one might suggest the influence of a delayed or recurrent labelling effect. This, of course, returns us to our original question regarding the action taken on the basis of labels--i.e., is it the label or the behavior that makes the difference? Angrist suggests an answer:

. . . the fact that the returnees were decidedly sicker than the community patients indicates that intrinsic features of the illness are of greater consequence in precipitating re-admission than are the variations in the way significant others perceive, evaluate or tolerate such illness (cited in Gove, 1970: 881).

Thus it seems that the labelling hypothesis definitely distorts major aspects of the diagnosis and treatment of mental illness. Gove summarizes his analysis as follows: "The available evidence. . . indicates that the societal reaction formulation of how a person becomes mentally ill is substantially incorrect" (1970: 881).



A study more supportive of the hypothesis under consideration is provided in the area of juvenile delinquency by Hackler (1970). The conceptual framework guiding this research effort is stated quite explicitly:

The model presented here is that children who are in a recognizable status (lower class, for example) are expected to behave in a predicted way. These predictions or anticipations on the part of the adult world are perceived by the child and are important to the development of his self concept. The perceived responses constantly indicate to the child the type of person he is and what is expected of him. This leads to self-categorizations and, along with the perceived expectations, influences the roles he will seek to play in an effort to behave in ways compatible with his imagined characteristics and capacities (1970 : 512).

Hackler operationalizes the above conceptual statement into the form of a causal model in which dominant and sequential relationships are specified. The model is then tested using questionnaire responses from 221 ninth-grade boys living in a medium-sized western U.S. city. Multiple measures of the causal variables included in the model are utilized and overall patterns are evaluated in assessing the findings of the empirical test.

What the findings reveal is considerable support for the conceptual framework proposed. Out of 36 separate test measures dealing directly with the major theme of the model (i.e., "that the perception of other's anticipations is the crucial variable in predicting delinquent behavior"), 22 of the measures indicate support for the central thesis. Looking at the entire model in its broader context, the findings indicate that 16 out of the 20 hypotheses derived from the model are favored rather than rejected. There is, then, reason to accept the sequential relationship suggested in the original conceptual statement,





and Hackler observes that ". . . one might conclude that the model approximates reality to some degree" (1970: 518-19).

Hackler's study is important, particularly given the fact that it rigorously interprets data relating to a set of theoretical speculations seldom dealt with on an empirical level. Yet, like any challenging piece of research, it can only tentatively resolve some questions at the cost of stimulating new ones. In the case of the current research, the questions to be asked do not easily lend themselves to resolution. Nettler states the problem as follows:

1. It is not denied that how people respond to us when we misbehave may affect the career of our conduct.
2. What is at issue is how much of the behavior to be explained varies with the response of others toward us (1969: 9c).

The question thus revolves around the problem of how much "others' definition of subject" and/or "perceptions of others' anticipations" adds to the subject's already established behavioral tendencies towards deviance. Posing the problem in this fashion involves the assumption that there are pre-existent behavioral tendencies among the potentially deviant. The accuracy of this assumption is supported by Gove's work on mental illness and by our earlier consideration of the psychological differentiae relevant to criminogenesis.

What we are suggesting, then, is that some portion (who knows how much?) of the relationships observed in Hackler's study may be a spurious artifact of accurate perceptions all across the board--i.e., others may accurately perceive that subject is objectively different, subject may accurately perceive that other is anticipating different behavior, and subject may indeed act in a consistently different manner. Given



these circumstances, part of what Hackler may have captured in his data is a set of perceptual "accompaniments" that accurately mirror, although they may not causally influence, consistent behavioral differences. A check on this possibility will obviously require continued research of the type and quality proposed in Hackler's utilization of the techniques of causal analysis.

In lieu of the type of future research suggested, the "labelling" or "reactions hypothesis" is most crucially to be evaluated as a therapy for the solution of the problems of deviance. For the labelling approach, in spite of the scattered empirical attention that it has received, continues to encourage the taking of various "optimistic risks" by agencies and officials of social control (cf. Nettler, 1969: 9d). Explicit in such risk-taking is the assumption that if we will react with positive expectations to the deviant, he will respond with behavior that conforms to these expectations. In its most extreme form, such therapies may be imposed upon subjects whose psychobiological make-up obviates the possibilities of responding with the desired behavior.

One study that provides a careful evaluation of the "reactions hypotheses," as therapy, is provided in Hackler's (1966) analysis of the Opportunities for Youth Project (OFY). A Seattle-based program for the prevention of delinquency, OFY was initiated in 1964 with a research design specifically aimed at evaluating the therapeutic results of manipulating the reactions of others to teen-age boys from high delinquency communities.

The research design forming the basis for this study utilized four experimental groups and one control group in each of four communities.





In two of the communities (A and B), experimental groups 1 and 2 both participated in work projects, with the only variation being the type of supervision received. Group 1 utilized "informal" supervisors who attempted to provide responses to the boys communicating anticipations for capable, responsible work characteristics. Group 2, on the other hand, utilized "formal" supervisors who provided less optimistic anticipations via rigid control practices. Experimental Group 3, in the same communities, offered individual work projects for the boys with minimal supervision; Experimental Group 4 supplied no work for the boys, but provided participation in a teaching machine testing program where the supervisors attempted to convince the boys that they anticipated behavior which was in keeping with an academically capable person; the control group supplied neither work nor the teaching machine experience.

In the final two communities (C and D), all groups remained the same except for Experimental Group 4; the experience with the teaching machine was not provided here and the boys were placed on an "active waiting list," giving members of this group more perceived access to jobs than members of the control group. Hackler summarizes the implication of the overall research design:

In general, the main theme of the program was carried out. Boys were placed in positions where it would be difficult for them to fail; success was practically guaranteed. We hoped that those boys would begin to see themselves as capable and adequate (1966: 159).

A variety of measures, 39 separate criteria in all, were included in an evaluation of the program's results. Yet, even when the criterion field was narrowed to a "chosen" 25 variables, the results of the program were not encouraging. Taking the sample of experimental subjects



as a whole, the findings simply do not point in the expected direction:

The control group seemed to show more change in a favorable direction than the four experimental groups. The differences are not large, but they show, clearly and painfully, that the action program had no impact on the boys (1966: 160).

Nevertheless, there are, apparently, scattered indications in Hackler's data that something may have worked. More specifically, although not statistically significant, patterns were found suggesting: (1) possible changes when the work program was combined with the teaching machine experience and/or when the teaching machine program alone was offered; (2) that the program may have had more impact on boys who had not been in trouble in school (Hackler, 1966: 163-64). The latter possibility in the Opportunities for Youth data, suggesting that "reaction" or "labelling effects" may differentially influence different types of subjects, brings us back to the major theme of this section.

What the data from Hackler's study may indicate is that "good" boys (as evaluated by school counselling records) are more easily influenced by the manipulation of others' reactions to them, than are "bad" boys. This interpretation of the findings is of course consistent with our argument that labels must, at least some of the time, correctly identify consistent behavioral tendencies that are resistant to alteration through optimistic and wishful redefinition. Thus there would seem to be wisdom in Holme's advice that "even a dog knows the difference between being stumbled over and being kicked"; and we are left to conclude that masochism may often be the sadder side of humanitarianism. Nevertheless, we have noted that redefining the situation may sometimes make a difference. The answer would seem to lie, then, in associating the labelling perspective with a taxonomy of subjects and situations so that





we know who may be "saved" from future difficulties by redefining those of the present (Nettler, 1969: 9d). Construction of such a taxonomy would not be an easy matter, but its availability might make the task of social control considerably less painful.

### Conclusions:

In the previous pages, we have attempted to assess the empirical warrant of the several working assumptions associated with the interactionist perspective in deviance. Conclusions, with reference to each of these assumptions, can now be stated.

- I. Psychological differentiae do, apparently, exist in a manner relevant to criminality. Although the degree of their causal importance is unknown, the interrelated concepts of extraversion, neuroticism, impulsiveness, response inhibition, and conditionability are all related to the production and explanation of criminality. Intelligence may also bear a varying relationship to criminality by type of offense.
- II. The use of empathetic procedures variously known as "role-taking" and "taking the other's viewpoint" are often productive of judgmental errors similar to those associated with everyday interpersonal perception. Thus there is a risk of naivety in the use of empathetic methods; but, even more important, if one accepts as a test of a knowledge-seeking tactic the ability to make accurate predictions, and if empathetic methods are then compared to actuarial techniques on the basis of their confirmed forecasts, the results



favor the latter approach.

- III. The interactionists are probably correct in assuming that behavior is mediated, to some degree, by thought-- nevertheless, questions remain: Which behaviors? In which situations? Are mediated by how much thought? Difficulties in answering these questions involve, among other things, the need to specify independent criteria for behavior, thought, and the processes that are said to mediate their relationship.

Our knowledge of the physiology of the human brain does not allow us to develop convincing solutions to the problems outlined above; nor does it allow us to predict specific behaviors on the basis of known thoughts. At the same time, cognitive psychologies have displayed a tendency to fill the vacancies in our neurophysiological knowledge with mentalistic redundancies. Analysts of deviance are thus encouraged to attend to those aspects of the thought-behavior problem that are more readily stated in the form of testable hypotheses.

- IV. A review of available evidence indicates that the manipulation of linguistic symbols is sometimes absent, frequently in accompaniment, and only occasionally a regulator in the process of perceptual discrimination. These results seem sufficient to suggest that the assumed verbal regulation of the perceptual process may often confuse correlation with causation. The result is a mistake in emphasis





which confuses verbal accompaniment of the perceptual process with verbal regulation of this same process, and exaggerates the role of the latter.

V. The predictive value of recorded verbalizations, in forecast of other measures of future behaviors, is shown to be low. Such findings do not encourage the interactionists' reliance on verbalizations as indicators of thought in the "explanation" of behaviors. Exclusive attention to verbalizations is thus seen as an inappropriate formula for the study of deviance.

VI. Others' definition of subject does influence subject's behavior, but we are again left with the question "how much?" Part of the answer to this question insists on the fact that labels do often correctly identify consistent and enduring behavioral differences. To answer the question in full, the labelling perspective must be associated with a taxonomy which locates those subjects and situations where redefinitions may alter behaviors.

In sum, we conclude that the neo-Chicagoans have often endorsed assumptions that are lacking in empirical support. But perhaps even more importantly, it seems that the remaining assumptions, often representing partial and vague truths, have led us in unprofitable directions. The correction of such errors may require that we reorient our approach to the field of deviance.

It is suggested that one possible model for the reorientation of our studies of deviance is to be found in the field of behavioral



psychology. The adoption of the behaviorist model, it should be noted, does not require a rejection of the awareness that "thought makes a difference" (cf. Skinner, 1963). What behaviorism does suggest is the operational definition of our concepts in observable and directly measurable terms. Admittedly, at this stage of our studies, these terms are primarily behavioral in their make-up. However, the long-run goal should be the formation of a behavioral taxonomy which could eventually be related to a cognitive taxonomy constructed in neurophysiological dimensions.<sup>9</sup> In lieu of the formation of such taxonomies, a preference is expressed for meeting the thought-behavior problem where it can be handled: on concrete grounds. There are, however, prerequisites to such an approach.

For the sociologist or criminologist there is the task of first classifying the behaviors to be explained so that they can be tabulated. Beyond this, these behaviors must be referred to the conditions in which they occur. Knowledge of these conditions may often amount to an awareness of the specific context eliciting the behaviors under study. Such knowledge, once gained, provides a basis for predicting the occurrence of the same or related behaviors in the future. In sum, the short-range benefits of such in-depth study of behaviors is an actuarial basis for the prediction of future events. And, as Fishbein (1966) notes, that is presumably why we sought to study attitudes in the first place. What behaviorism provides, then, is a short-cut to our goal.

At the same time, however, we must re-emphasize that the application of these behavioristic principles in no way denies the possible background of thought for such behaviors. Thus, Creelman (1966) locates





in reinforcement theory an explanation for symbolizing processes that may elicit behaviors. This explanation has as its base the orienting reflex: the complex patterning of innate physiological and behavioral reactions to novel stimuli. It is via the orienting reflex that the organism first makes contact with the environment. Associations formed at this stage take place through the primary sensory signalling system with the new connections superimposed upon unconditioned reflexes or old and established conditioned reflexes. Higher order conditioning, corresponding to what the mentalists refer to as cognition or symbol manipulation, occurs when the secondary (symbolic) signalling system enters the picture. Here words and other varieties of symbols take on reinforcing qualities through previous association with conditioned and unconditioned stimuli. Acting as secondary reinforcers, these symbols make possible the control of behavior from within the organism. This process of control continually is altered through an ongoing dynamic interplay between the primary (sensory) and secondary (symbolic) signalling systems. In this sense, the organism is engaged in a continuing sequence of controlling his responses via the sensory input received from his encounters with external conditions.

All of what has just been described in behavioristic terms must also have an underlying neurophysiological explanation. Miller, et al., (1960), in fact, argue that such a neurophysiological explanation must necessitate a modification of the behavioristic model. Regardless of whether this may be the case, however, the behavioristic model does currently provide a parsimonious scheme that allows us to deal concretely on an experimental level with the thought-behavior problem. It is



here suggested that in the study of deviance we might well benefit from the behaviorist approach, at least until the field of neurophysiology provides a more comprehensive base for building a generalized theory explaining the relationship of thought to behavior.

In the following chapter, we will consider the interactionist and the behaviorist explanations as they are applied to one particular variety of deviance--opiate addiction. It will then be possible to demonstrate, by means of example, how the use of the assumptions of symbolic interactionism may hamper our efforts to explain deviant behavior.



## FOOTNOTES

<sup>1</sup>A defect of such reviews should be noted. There is a tendency to indifferently combine both careless and competent research efforts in reaching more "comprehensive" conclusions.

<sup>2</sup>There have, of course, also been negative evaluations of the Gluecks' work. Among the more useful of these critiques are those contributed by Shaplin and Tiedeman (1951) and Cohen (1961). Excerpts follow:

Shaplin and Tiedeman: The prediction studies of the Gluecks represent only the first stage in the development of prediction data: the establishment of criteria. After this, attention should be given to the problem of combining the criteria in the most efficient manner. The Gluecks' method is not the most efficient, but its practical advantages cannot be ignored (1951: 548).

Cohen: The Gluecks have labored many years and with incredible assiduity to unravel juvenile delinquency. It has been the judgment of most sociological commentators that all this dedicated and feverish unravelling has left us with a very tangled skein. . . . It is true that the Gluecks' writings have been vulnerable to criticism, on both theoretical and methodological grounds. The same can be said, however, of much research under sociological auspices, and many of the same criticisms, especially with regard to the selection of samples, are often relevant. . . . [Thus] the methodology of Unravelling and of Physique and Delinquency seems to the reviewer to be not so shoddy, at least in comparison to the methodology of most sociological studies, that the resultant data are to be dismissed as not worthy of study and an effort to make sociological sense of them, even if, like the reviewer, one cannot embrace the theoretical orientation of the Gluecks (1961: 272-73).

<sup>3</sup>A deficit of studies utilizing the MMPI concerns the level of validity they achieve; the problem involved is the apparent incidence of both social desirability and social undesirability effects in responses to inventory items. Thus D. J. West reports:





Despite. . . trends, the findings were very variable with numerous inconsistencies. . . . In fact, the most significant difference statistically between delinquents and non-delinquents was the high scores given by the former on the validity scales which are meant to detect unreliability of response. An implausible excess of claims to virtue ('faking good') and a suspicious excess of self-denigrating responses ('faking bad') were both features of the delinquent group (1968: 145).

Nevertheless, West still regards the findings reported by Hathaway and Monachesi as having considerable importance.

. . ., in their survey, as well as in numerous other surveys of samples of established delinquents, such as that of borstal youths by Gibbens. . . it was shown that delinquents tend to score unusually high on the so-called psychopathic deviate scale of the M.M.P.I.. This significant difference in the pattern of delinquent responses implies some kind of difference in the personality of delinquents as a group, and the kind of peculiarity displayed falls in line with the stereotype of the aggressive, anti-social character formation described by clinical workers (1968: 169).

In spite of the problems noted, then, there appears to be adequate reason to attend carefully to studies utilizing MMPI scores.

<sup>4</sup>The difficulties associated with acceptance of the conditionability postulate are outlined by West:

In particular, measures of conditionability applied to humans are relatively new, and results are often inconsistent. The techniques are not very easy, and many extraneous influences, such as motivation, distractions, and physical fitness, may possibly interfere with the responses. It has yet to be established how far conditionability can be regarded as a unitary trait, or to what extent a given individual varies in his speed of conditioning according to the kind of situation in which he is placed (1968: 146).

<sup>5</sup>Nettler suggests the myopic view of the thought process that exclusive attention to verbalizations may provide:

If one agrees with Langer and other philosophers that mental life ("thinking"?) is more than what appears in words, the symbolic repertoire is seen as inadequate to express those feelings that are part of "how-we-think," and "what-we-say" becomes its poor representative (1970b: 56).



<sup>6</sup>There are empirical indications that the interactionists' assumptions regarding verbalization, symbolization, and consciousness have misled us in directions beyond those specified in the preceding discussion. For example, Mead goes so far as to suggest that,

When, in any given social act or situation, one individual indicates by a gesture to another individual what this other individual is to do, the first individual is conscious of the meaning of his own gesture. . . . (1934: 158).

Current studies of non-verbal communication would seem to seriously undermine the credibility of such assumptions. The research of Ray Birdwhistell is a case in point.

Birdwhistell (1968; 1970) has observed that social scientists have too long labored under the false assumption that all communication flows on the verbal level. It is argued here that communication is a multi-channel phenomenon consisting of independently merged infra-communicational systems. Of special interest to Birdwhistell is the language of body motion; to study this type of communication, Birdwhistell developed a science of the body behavioral language--kinesics. In a parallel manner, Hall has emphasized the human use of space arrangements as a transactional medium and has developed what he calls the study of "proxemics." And, on a more abstract level, Westcott is attempting to order the various channels of communication and to determine their structural relation to one another. The framework of Westcott's discussion is conceived as "streptistics" (Birdwhistell, 1968: 379-85).

There is an assumption-implicit to each of these fields of study that places them in opposition to the school of symbolic interactionism. Birdwhistell states this fundamental assumption in a rather succinct manner:

All Kinesic research rests upon the assumption that, without the participant's being necessarily aware of it, human beings are constantly engaged in adjustments to the presence and activities of other human beings (1970: 48, emphasis altered from the original).

Birdwhistell extends this position by arguing that mentalistic conceptualizations of communication run the risk of becoming "prescriptive rather than descriptive" (1970: 66). In sum, Birdwhistell seems to reject the notion of human interaction being a purely "symbol-mediated" process.

An indication of the variety of findings already accumulated by other investigators interested in the study of body motion is available in the work of Ekman (1964; 1965), Rosenfeld (1966), and Mehrabian (1970). Of the three, Mehrabian's work is the most comprehensive.





Mehrabian has summarized a wide range of findings relating to non-verbal and primarily body behavioral communication. Perhaps the most interesting of these findings involves the types of implicit movements and gestures which can indicate liking, confidence, or higher status on the part of the communicator. For example, Mehrabian notes that posture can reflect a person's tension state and thereby communicate his liking of other, as well as possible status differences between actor and other in the interactional situation. In operational terms, greater degrees of postural relaxation are denoted by moderate asymmetry in the placement of arms or legs, slight sideways lean while seated, and a somewhat reclining position. The general rule that emerges from these findings is that to communicate liking one must avoid both extreme asymmetrical or symmetrical postures; in other words, to communicate liking one should demonstrate moderate relaxation in his body positioning. Similar patterns are found in situations of status difference and respect. Thus, a person is usually extremely relaxed when talking to someone of lower status and only moderately relaxed when the communicatee is someone of higher status. From this, Mehrabian suggests that to communicate respect one should demonstrate slightly less relaxation than may feel appropriate in a particular situation (1970: 68-69).

Spatial factors are also apparently important in communicating liking, respect, and status. Thus Mehrabian reports that the more people like one another, the more they orient themselves to each other and the closer they get in terms of physical proximity. In this context, people of equal status assume an indirect orientation towards each other, standing close but side to side. When a status differential exists between the actors, however, the arrangement shifts to a direct orientation, with the lower status person aligning himself face-to-face towards the high status other and with greater physical distance maintained. In addition, it is noted that eye contact increases with liking and that one looks more into the eyes of an important or high status person than a lower status individual. Mehrabian concludes from these findings that respect can be communicated with distance, direct orientation, and eye contact (1970: 69).

There are many additional findings covered in Mehrabian's summary, but by now we assume that our point has been safely made: much that we "signify" is communicated by non-verbal means, on the sign level of expression, and in a non-conscious manner. Interaction, it seems, is a process involving both symbols and signs--with the quantity or quality of human behavior belonging in either category remaining an empirical unknown.

<sup>7</sup>Easily the most graphic, although understandably not the most rigorous, comparison of 'what people say' with 'what people do' is provided in Polsky's (1969) discussion of incest:

. . . the widespread use of a term for a deviant role is not always a good clue to the prevalence or even the existence of role incumbents. An example: In the American kinship system, the three basic types of



incest are brother-sister, father-daughter, and mother-son. We have a common term, originating in Negro subcultures and now part of general American slang, to designate one partner in the third type ("motherfucker"), and no special terms for the other types. But the facts of American incest are the reverse of what the language might lead one to believe: brother-sister and father-daughter incest are frequent, whereas mother-son incest is so extraordinarily rare that the staff of the Institute for Sex Research, when I queried them about this a few years ago, had found only one case that they regarded as genuine (1969: 123).

<sup>8</sup>Although the relationship reported is highly statistically significant, Deutscher indicates the inconclusiveness of Defleur and Westie's findings:

Unfortunately, in spite of the fact the the probability is 99 out of 100 that the distribution in the study. . . is attributable to something other than chance (presumably to the relationship between attitude and behavior), the fact remains that 30 per cent of the cases are deviant: 14 of the 46 subjects were either prejudiced people who showed a high level of willingness to sign releases or unprejudiced people who showed an unwillingness to sign releases (1969: 38).

<sup>9</sup>Glimpses of the type of knowledge such a cross-classification of behavioral and cognitive taxonomies might reveal can now only be approximated in a rather crude fashion by reference to some ingenious and well-known groups of experiments.

One such group of experiments, of particular significance to the field of deviance, is fully explored in Chapter Three of this thesis. Here, the class of behavior to be explained is opiate addiction. Experiments in this area have sought to establish whether the patterns of addiction observed in humans can be replicated in animals. The use of animals in these experiments represents a very crude control of the level of cognition necessary for the acquisition of addictive behavior. Ideally, it would be possible to gain a more discriminative control over the level of cognition present in the experimental situation; however, as indicated earlier, the neurophysiological knowledge necessary to differentiate cognitive operations at very precise levels is as yet unavailable. Nonetheless, this limitation has not prevented the accumulation of considerable evidence about the role of cognition in opiate addiction. The reader is referred to Chapter Three for a detailed discussion of these findings.

Another group of experiments, by DiCara and Miller, provides further indication of the benefits future research may bring to considerations





of the thought-behavior problem. DiCara and Miller have been concerned with the assumed differences between operant and classical conditioning. DiCara (1970a) notes that classical conditioning has traditionally been regarded as an inferior type of learning, because of its presumed involuntary nature. In the classical learning arrangement, the conditioned stimulus is presented in accompaniment with an innate unconditioned stimulus that normally produces a certain innate unconditioned response; given these conditions, over time, the conditioned stimulus will alone begin to produce the same unconditioned response. It is important to note that in the classical arrangement the stimulus and response originally associated were bound in a natural relationship. Operant conditioning, however, incorporates no such limitations; rather, operant conditioning allows voluntary control over any stimulus-response arrangement. The mechanism for this type of learning is simply the reward of any desired response consequent to a conditioned stimulus. Because of this flexibility, operant conditioning has been regarded as the superior of the two types of learning.

Miller (1969) maintains that the distinction between classical and operant conditioning as two basically different phenomena is false. Instead, Miller argues, both types of learning are merely different manifestations of the same phenomena under different conditions. What is involved here, then, is a quarrel over taxonomic categories. The traditionalists would seem to be promoting two behavioral categories based on the distinctions described above. Miller, on the other hand, recommends one larger category, while allowing for behavioral variation within this category according to situational conditions. The resolution of this quarrel lies in experimentation.

In support of his position, Miller has experimentally challenged a long-standing belief that instrumental learning is possible only in the cerebro-spinal system and, conversely, that only classical conditioning can be used to modify response patterns in the autonomic nervous system (Miller, 1969: 435). What Miller has succeeded in doing is to produce learned visceral responses, using operant procedures, where only classical procedures had been known to work before (DiCara, 1970a; Miller, 1969). In short, it now has become possible to modify through reinforcement techniques such visceral responses as heartbeat and intestinal contractions.

Although these experiments were obviously impressive and lent considerable support to Miller's position, the traditionalists seem to have regained the initiative with more recent findings. DiCara (1970b, with Braun and Pappas) has contributed a study which experimentally explores the possibilities for operant and classical conditioning in rats with and without a neo-cortex. The results of this experiment are striking in that the intact neo-cortex appears as a functional necessity for the operant conditioning of cardiac and gastro-intestinal responses. At the same time, the success of classical conditioning of these responses is apparently not dependent on the presence of the neo-cortex. The implication of these findings is that the taxonomic distinction between operant





and classical conditioning is correlated with a corresponding distinction in cognitive capacities.

Once again, as in the experiments involving opiate addiction, the distinction in cognitive capacities used in DiCara's experiment is quite crude. In no way does the resulting knowledge include the necessary information allowing us to predict specific operant behaviors. Yet, such experiments do provide a glimpse of what may be possible when detailed behavioral taxonomies (phrased with reference to particular situations and conditions) are eventually correlated with sophisticated cognitive taxonomies (constructed along neuro-physiological dimensions).



### CHAPTER THREE

#### COGNITIVE ASSUMPTIONS IN THE EXPLANATION OF OPIATE ADDICTION

They call me, they call me a 'junko' 'cause I'm loaded  
all the time, . . .  
My brother, my brother used a needle, and my sister  
sniffed cocaine, . . .  
My mother, my mother she tol' me, an' my father tol'  
me too,  
'That junks a bad habit, why don't you leave  
it too?'  
My sister she even tol' me, an' my grandma tol'  
me too,  
'That usin' junk, pardner, was goin' be the death  
of you.'

--Junker Blues  
Champion Jack Dupree

Words and wishes to the contrary, opiate addiction persists. Yet, as sociologists, we still act as if definitional thoughts could yield alternative behavioral consequences. In short, we act as if a complex cognitive process is the mediational agent in the abuse of opiates. As the song suggests, we should--by now--know better.

But there is theoretical consistency, if not pragmatic validity, to the attitudes held by sociologists. To understand this consistency we must look to sociology's sister science--psychology.

#### WHEN BIASES COLLIDE

There exists within the discipline of psychology a continuing theoretical conflict between the 'behaviorists' and the 'mentalists.'





The former have preferred a behavioral psychology focussing attention on those overt activities available for direct observation and measurement; the latter have favored a cognitive psychology emphasizing covert 'behaviors' often 'known' only by phenomenological experience and indirect measurement. Sociology has become a party to this dispute through the work of George Herbert Mead.

Mead attempted to bridge the theoretical gap between the behaviorists and the mentalists, but the impact of his work has been a wholesale acceptance of the cognitive bias. The bias finds its expression in a version of the social psychology that Mead called "social behaviorism" (1934b: 120).

In social psychology we get at the social process from the inside as well as the outside. Social psychology is behavioristic in the sense of starting off with an observable activity--the dynamic, ongoing social process and the social acts which are its component elements--to be studied and analyzed scientifically. But it is not behavioristic in the sense of ignoring the inner experience of the individual--the inner phase of that process or activity. On the contrary, it is particularly concerned with the rise of such experience within the process as a whole (1934b: 121-22).

The title attached to the Meadian style of social psychology has since changed from "social behaviorism" to "symbolic interactionism" (Blumer, 1937),--a school name that accurately reflects a final purging of the behaviorist viewpoint and the complete acceptance of a cognitive perspective.

Sociology thus finds itself taking a polarized position in an unresolved dispute; this position requires the acceptance of a tenuous and untested premise. Blumer states the premise for us:

In their association human beings engage plentifully in non-symbolic interaction as they respond immediately and unreflectively to each other's bodily movements, expressions,



and tones of voice, but their characteristic mode of interaction is on the symbolic level, as they seek to understand the meaning of each other's action (1969: 8-9, emphasis added).

It is not within the scope of this discussion to test the general validity of the above assumption. Rather, we will be satisfied with the accomplishment of two limited objectives. First, we will investigate in detail one variety of behavior--opiate addiction--and its relationship to the cognitive or symbolic process. Here we seek a resolution, in the specific area of opiate abuse, to the dispute between the behaviorists and mentalists. Second, we will attempt to convince sociologists that the assumption of a cognitive basis for human activity must be re-examined in each of the wide ranges of behavioral situations. Indeed, at least one of the challenges for sociology is to determine just exactly what varieties of behavior are mediated by precisely how much cognition. The completion of both objectives will reveal a flaw in the extreme theoretical position taken by symbolic interactionism.

#### NORTH AMERICAN CHEMICALS OF COMFORT

Our discussion will be restricted to drugs derived from or equivalent to opium. Opium in its basic form is seldom used in North America; preparations are tedious and the dosages required are large. Morphine and its derivatives are the current chemicals of choice. First encounters with morphine involve several characteristic effects: nausea, vomiting, itching, respiratory and cardiac retardation, reduced food and sexual appetite and a possibly pleasant phenomenon known as 'being on the nod.' These symptoms usually disappear with the onset of



tolerance; however, miosis, constipation and reduced sexual activity may persist.

Intravenous injection (i.e., 'shooting') is a stylistic preference among North American addicts. The sensational correlates immediate to this means of administration are flushing, itching, dizziness and a gastric spasm--often described as a 'flash' or 'jolt' analogous to the intensity of sexual orgasm. This initial 'jolt' does not diminish with the acquisition of tolerance.

If satisfactory amounts of morphine are available to the addict, physical hygiene does not deteriorate, performance on psychological tests remains consistent, perception is not impaired, and various 'anxieties' are reduced.

Abrupt abstinence from morphine, after acquisition of tolerance to large doses, produces a characteristic sequence of symptoms. Ten to twelve hours after the last injection, the process begins with yawning, perspiration, and other physical discomforts. Disturbed sleep follows. Eighteen to twenty-four hours after the initiation of abstinence, the addict awakens to an intensification of the initial symptoms, coupled with the experience of 'goose flesh' (i.e., 'cold turkey'), alternate hot and cold flashes and severe pains and/or twitches in the legs. The symptoms grow in intensity and are accompanied by continuing insomnia and gastro-intestinal disturbances. At the end of two days these symptoms have reached their peak. By the third day a decline in abstinence effects begins and at the end of ten days most of the symptoms will have disappeared.

Other opiates and morphine derivatives produce the same quality





of effects characteristic to morphine; however, substantial quantitative variation exists among responses. The 'popularizer' among morphine derivatives is diacetylmorphine (heroin). Heroin is more potent than morphine, but its effects are of shorter duration, necessitating an increase in the frequency of injection. Codeine is a less popular opiate. Its effects are less desirable, the doses required are large, and its cost is prohibitive.

Among the synthetic equivalents to the opiates are Meperidine (demerol) and Methadone. Demerol is the preferred choice among the synthetic equivalents. However, its excitatory effects are short-lived and the acquisition of tolerance is relatively poor. Methadone is a sometimes satisfactory substitute for morphine among confirmed addicts while its addiction potential for the non-dependent population is relatively low. The withdrawal effects associated with dependence on Methadone are minimal; as such, it is often recommended for the treatment of opiate addiction (Seevers, 1958: 242-44).

Our range of 'pleasures' delineated, we can proceed to explanations.

#### THE MENTALIST EXPLANATION

The one distinctively sociological explanation of opiate addiction has been developed by Alfred Lindesmith (1938; 1947; 1968). The theory's bias is cognitive and its framework is consistent with those assumptions characteristic of symbolic interactionism.

Lindesmith has constructed his theory using the method of analytic induction. A stirring description of Lindesmith's methodology is provided by Blumer.



Dr. Lindesmith has endeavored to realize the perfect form of scientific knowledge, i.e., propositions conceived as universal in character that would permit the discernment of exceptions, and thus make possible the perfecting or refinement of the propositions. No mere correlation, no mere partial proposition, no mere suggestive declaration would suffice. He has aimed to develop. . . an interpretation that. . . covers all the instances of drug addiction. His achievement of this aim stamps his work with the distinction rarely attained in social science research (Lindesmith, 1947: ix).

It should be noted at the outset that the requirement of universality, the central feature of Lindesmith's methodology, predetermines the construction of a theory characterized by retrospective description (Robinson, 1951; Turner, 1953; Schur, 1965). Thus a major criticism of the theory has been that it is predictively impotent. While we agree with this criticism, we will reserve consideration of its implications for Chapter Four. In the current discussion, we will be concerned with the accuracy of Lindesmith's conceptualization of the learning process associated with opiate addiction.

Lindesmith's search for a universal explanation began with the awareness of a puzzling fact. Apparently, not all persons who experience the prolonged use of opiates and the resulting physical dependence become addicted (1968: 3). This selective avoidance of addiction primarily occurs in the context of prolonged hospitalization. To conceptually reflect this differential reaction to sustained usage of opiates, Lindesmith distinguishes 'addiction' from 'habituation.'

Addiction may be defined as that behavior which is distinguished primarily by an intense, conscious desire for the drug, and by a tendency to relapse, evidently caused by the persistence of attitudes established in the early stages of addiction. Other correlated aspects are the dependence upon the drug as a twenty-four-hour-a-day necessity, the impulse to increase the dosage far beyond bodily need, and the





definition of one's self as an addict. This complex of behavior will hereafter be referred to as "addiction," and the organism which exhibits it will be called an "addict." The term "habituation," on the other hand, will be used to refer to the prolonged use of opiates and to the appearance of tolerance and withdrawal distress, when it is not accompanied by the behavior described above as addiction behavior (1968: 64-65, emphasis added).

From this definition, Lindesmith extracts the essential features of the universal learning process characterizing addiction. Lindesmith seems to see this learning process as involving four steps: (1) some variety of social-psychological tension stimulating initial usage, (2) physiological readjustments of the organism resulting in tolerance and physical dependence, (3) physiological experience of withdrawal distress combined with a 'proper' cognitive identification of this experience and (4) social-psychological dependence.

The 'critical' stage, according to Lindesmith, is the third step of the addiction process. Lindesmith argues that the subject cannot become addicted unless he cognitively identifies the source of his withdrawal distress and the means of its relief (1958: 599).

It is therefore not surprising that the behavioral consequences of the withdrawal experience do not produce addiction except when the experience is cognitively grasped in a particular way; that is, when it is understood by the subject (1968: 95, emphasis added).

Carrying his argument further, Lindesmith insists that conceptualization and understanding are cognitive processes made possible by the manipulation of language symbols. Since these capacities are only thought to be found in man, addiction is judged to be a distinctively human phenomenon. On this point, Mead would have agreed,

. . . we do not find in any animal behavior that we can work out in detail any symbol, any method of communication, anything that will answer to. . . different



responses so that they can all be held there in the experience of the individual. It is that which differentiates the action of the reflectively intelligent being from the conduct of the lower forms; and the mechanism that makes that possible is language (1934b: 186).

If we accept the Lindesmith thesis, and if we concur with Mead's conclusions, then we must be prepared to argue that animals cannot be addicted. The behaviorists have attempted to test this proposition.

### THE BEHAVIORIST EXPLANATION

The behaviorists have shown that animals, much like human beings, become addicted to opiates. These demonstrations emerge from experiments based on both Hullian classical conditioning and Skinnerian operant conditioning models. A great deal of this work can be summarized as moving towards a "two-factor learning theory" of relapse, suggested by Wilker (1965). Both factors in this theory describe directly measurable and overt activities. These factors are: (1) classical conditioning of physical dependence through repeated temporal contiguities between a specific environment and the occurrence of morphine-abstinence phenomena; and (2) reinforcement of instrumental activity (morphine-acquisitory behavior) through repeated reduction by the drug of such abstinence phenomena as develop during intervals between doses (1965: 88-89). Both these factors, at first indirectly and only later directly, have been involved in a wide variety of animal experiments going back over forty years.

Tatum, Seevers and Collins (1929) were among the first to attempt the replication of human addiction in animals. Their modest conclusion was that a "condition closely portraying human addiction could be



arrived at in the monkey" (Seevers, 1936: 147). The basis for establishing addiction in these experiments was crude, only indirectly involving the second factor of Wilker's relapse theory. The researchers sought to condition addictive behavior through "the establishment of a positive association between the needle and the relief of symptoms" (Seevers, 1936: 148). As yet, no attention was given to the monkey's active involvement in the acquisition of the needle. It is important to note, however, that by emphasizing the incidence of a positive association, rather than the occurrence of a symbolic cognitive process, the researchers permanently altered the study of addiction.

The next important conditioning experiments in the area of addiction were carried out by S. D. S. Spragg (1940a). Responding partially to Lindesmith's 1938 article on opiate addiction, Spragg was precise in his discussion of the factors involved in his work.

The subjects of the present investigation did not have the nature of morphine addiction 'explained' to them; nor did it appear in them as a 'social phenomenon' in the sense in which Lindesmith has used the term.

The appearance of addiction in these animals involved essentially the formation of an association between the hypodermic injection and the alleviation of withdrawal symptoms . . . this association and not anything 'social' (in the sense of societal) is the essence of morphine addiction (1940a: 122).

But Spragg's work is important for more than its direct rebuttal of the Lindesmith thesis; his experiments also contain crucial innovations allowing the incorporation of both factors found in the relapse theory of addiction. The first factor, the conditioning of physical dependence in a specific environment, was incorporated by administration of morphine injections twice daily in a special injection room.





Beyond this, after each dose the chimpanzees were kept in an injection box for at least ten minutes, so that the drug's effects could be associated with that particular setting. The second factor, reinforcement of instrumental activity by reduction of abstinence effects, was built into the experiment by first conditioning the chimps to open boxes with a stick, one box containing food, the other a hypodermic syringe. The chimps were fed if they opened the first box and injected if they opened the second.

The results of Spragg's experiments were impressive. Three of the four chimps demonstrated strong addictive behavior: they were eager to go to the injection room, solicitous of injections and excited to enter the injection box. On the negative side, however, only one chimp survived an abrupt withdrawal period, and this single survivor did not relapse into addiction.

The next series of important experiments were carried out by Beach (1957a). Beach sought the answers to three related questions: (1) Is drug addiction peculiar to man and the primates? (2) Does drug addiction require the manipulation of language symbols? (3) Is the learning of addictive behavior dependent only on drive reduction, or is there also some pleasurable and euphoric effect involved? (1957a: 104).

The procedures followed in Beach's experiments paralleled those used by Spragg. Thus the findings emerging from these experiments are related to the two-factor theory of learning, as discussed above. The results of the experiments indicate that rats can be addicted in a manner similar to primates and humans. That this behavior is fully comparable to opiate abuse in humans is further supported by the finding



that rats spontaneously recovered their addictive activities (i.e., morphine-acquisitory responses) after the complete disappearance of abstinence effects (i.e., withdrawal). In other words, the rats relapsed into addiction. It will be recalled that relapse was not observed in Spragg's research. Beach's experiments thus supply convincing new evidence in favor of rejecting Lindesmith's hypothesis that manipulation of language symbols is necessary for the development of morphine addiction. A final, but extremely important finding in Beach's study is that morphine's euphoric effect, as well as its reduction of withdrawal distress, reinforces the learning of addictive behavior (1957a: 110-11). This last discovery implies an explanation for the usage of drugs during the early stages of the addict career--i.e., before withdrawal effects have been experienced. This finding may also help to account for relapse among 'cured' addicts.

A number of recent experiments by Nichols and Weeks have taken the final steps towards specifying experimental conditions that render animal addiction, for all observable purposes, synonymous with the drug experience in humans. Nichols' 1965 experiments on rats should be considered first. Puzzled, like Lindesmith, by the contrast between hospital 'habituation' and street 'addiction,' Nichols attempted to design an experiment that would reveal the source of these differential responses to the opiate experience. In other words, Nichols concerned himself with determining those situational conditions coincident with the relapse behavior characteristic of human addiction. He hypothesized that ". . . such behavior is likely to develop in subjects who initiate the action of taking drugs and administer opiates to themselves"





(1965: 80, emphasis added). Nichols thus went a step further than previous researchers who only allowed their subjects to initiate, but not themselves carry out, the administration of opiates. This experiment therefore realizes the full incorporation of the second factor characterizing the relapse theory of addiction. Nichols finds this factor consistent with the basic principle of operant conditioning.

It is more than coincidence that operant conditioning, the most effective means of changing behavior known to experimental psychologists, entails some subject-initiated response (1965: 80).

Nichols goes on to point out that a passive recipient of opiates (e.g., the hospital patient) has not been engaged in operant conditioning. The subject must first take action before that action can be reinforced.

What has been said for humans applies also to animals. And Lindsmith's predictions notwithstanding, animals can be 'turned on' to active and persistent use of morphine. Nichols demonstrated this fact convincingly in a number of experiments. Using a "self-injection" device developed especially for rats, Nichols trained an experimental group to actively administer their own doses of morphine. This experimental group was then compared to a control group who were passive recipients of equal amounts of morphine. The experimental group of "active administrators" maintained their opiate-directed behavior beyond the point when withdrawal effects disappeared. The control group of passive recipients, on the other hand, continuously showed no opiate-directed behavior (1965). Nichols' hypothesis was therefore confirmed. Similar experiments, again using self-injection as the means of administration, were also successfully completed by Weeks (1964). One may



conclude that the factors discriminating between addiction and habituation are those constituting the physical circumstances of intake; in specific terms, the more active the subject's concrete involvement in the administration of the drug, the more sustained will be his opiate-directed behavior.

We have followed the behaviorist explanation of addiction through a long series of experiments. Each group of experiments has carried us a step closer to the replication of human addiction in animals. We have seen that any rat, like Everyman, can become an addict. Our conclusion is that the cognitive viewpoint is in error when it assumes that addiction is a distinctively human process. But Lindesmith remains skeptical.

#### A MENTALIST REBUTTAL

Lindesmith finds several inadequacies with the behaviorists' experiments.

- (1) Spragg is charged with committing the fallacy of ascribing human qualities to animals--e.g., the assertion by Spragg that chimpanzees 'desired' morphine injections.
- (2) On the basis that Spragg's chimpanzees failed to relapse, it is suggested that they are actually 'habituals' and not 'addicts.'
- (3) The tendency of relapse behavior in animals to eventually subside after extended periods of abstinence is considered a serious blow to the behaviorist explanation.
- (4) A disparity between the findings of Spragg and Nichols with regard to relapse is observed.



- (5) The behaviorists are criticized for a failure to relate human and infra-human differences in intelligence to the manner in which conditioning or reinforcement operates.
- (6) It is alleged that one cannot adequately study opiate abuse without considering 'self-concept' and its relation to addictive behavior (1946: 39; 1968: 181-88).

These criticisms are deserving of response.

#### THE BEHAVIORIST REJOINDER

A behaviorist response to Lindesmith's list of inadequacies can be arranged in point-by-point form:

- (1) While it is difficult to 'know' if Spragg's chimpanzees actually do 'desire' their injections, we can also only infer that Lindesmith's humans 'crave' their 'fixes.' We are on safer ground when attending to the overt behaviors of both species. And at least with reference to the consumption of opiates, the behaviors are quite similar.
- (2) Three of Spragg's four chimpanzees died during an abruptly instituted period of abstinence. Thus three of the experimental subjects were not available for a test of relapse behavior. The one chimpanzee that did survive abstinence failed to relapse into addiction. There are at least two reasons for not being overly concerned with this finding. First, it represents only one animal's response to addiction. Since we are not restricted by Lindesmith's fetish for universality and since other experiments (Nichols, 1965;





Weeks, 1964; and others) demonstrate conclusively that animals do relapse under appropriate circumstances, we remain undismayed by this single exceptional case. Second, there is a viable explanation for the failure of Spragg's chimpanzee to relapse. This animal only initiated, but did not itself carry out, the injection of the morphine used in the experiment. Since this second factor has been judged instrumental to the operant conditioning of addictive behavior (Weeks, 1965), we should not be surprised that only an habitual response pattern was observed in the animal.

- (3) It is true that relapse behavior in animals does show a tendency to diminish following extended periods of withdrawal. It should be clear, however, that the relapse behavior occurs long after withdrawal symptoms have disappeared. Nevertheless, it may be that the association formed in addiction is weaker for animals than for humans. This reflects, of course, a quantitative and not a qualitative difference. Perhaps a pertinent fact to add here is that many human addicts are similarly observed to stop using opiates as they advance in age (Nichols, 1965: 88).
- (4) As indicated in point number two, the disparity between Spragg's and Nichols' findings on relapse may be explained by the different techniques used in the administration of drug doses. We regard Nichols' innovations as an explanatory insight into the differences between habituation and addiction.



- (5) Lindesmith proposes the following thesis to account for assumed differences in reactions to drug use and withdrawal distress: "The brighter human subject is likely to have acquired sufficient knowledge of drugs so that he grasps the situation even when he is the passive recipient; the animal, on the other hand, because he is not nearly as bright, has difficulty making even some of the most rudimentary associations" (1968: 187). A correlation is hypothesized here between intelligence and the capacity for addictive behavior. Spragg (1940b) exposes this hypothesis in a study using human subjects. Here it is shown that deviations in intelligence, as measured by intelligence tests, are not related to the incidence of addiction.
- (6) All of our favorite children's stories to the contrary, an animal is usually not judged to possess a sense of self. Nonetheless, we have seen that animals do become addicted to opiates. Such research, then, fails to demonstrate that self-conceptualization is a necessary causal component in the etiology of addiction. A self-concept may be necessary to other varieties of behavior (we will leave this question to other research efforts), but it does not seem essential to opiate abuse. We would conclude instead that a positive association between the reduction of withdrawal symptoms and the acquisition of opiates is the central determinant of addiction. Crucial to this positive association is subject's initiation of the acquisition process and self-injection of





the drug. These, rather than self-concept, are the facts of addiction.

The cognitive bias characteristic of the interactionist explanation of opiate addiction should by now have been discredited. Instead, and in spite of this behaviorist critique, it has become the conceptual cornerstone in recommendations for public policy formation.

#### SELF-CONCEPT AND PUBLIC POLICY

The notion of self-concept acts as a metaphorical springboard allowing the explicator of opiate addiction to enter the world of public policy formation. It is argued that modified laws, operating through the medium of altered self-concepts, produce changes in behavior. This approach to the problems of deviance is often known as the "labelling perspective."

George Herbert Mead is credited with having anticipated the labelling perspective (Lemert, 1967: 42). For Mead, "The majesty of the law is that of the angel with the fiery sword at the gate who can cut one off from the world to which he belongs" (1918: 587). He observes that the law isolates deviants, branding them by means of a stigmatization process. The Meadian conclusion is that ". . . a system of punishments assessed with reference to their deterrent powers not only works very inadequately in repressing crime but also preserves a criminal class" (1918: 581).

The labelling perspective has recently gained great popularity. Its advocates are many and their influence is growing. Perhaps the most important work in this area is that done by Edwin Lemert. As the



reader will recall from Chapter One, Lemert proposes that there are two varieties of deviance: primary and secondary. Primary deviance is described as polygenetic in that it arises out of a variety of social, cultural, psychological and physiological factors (1967: 40). However, ". . . primary deviation has only marginal implications for the status and psychic structure of the person involved" (1967: 40). In other words, primary deviation has little impact on the self-concept.

Secondary deviance, on the other hand, involves the problems arising from societal reaction (e.g., enforcement of laws) to primary forms of deviance. It is here that problems of self-concept emerge.

They become central facts of existence for those experiencing them, altering the psychic structure, producing specialized organization of social roles and self-regarding attitudes (1967: 40-41).

As indicated earlier, the assumption emerging from this viewpoint is that a change in societal reaction will be accompanied by a change in behaviors. It is assumed that we are in possession of a formula allowing us to halt deviance at its primary stages.

A number of authors have used the labelling perspective in the analysis of opiate addiction, but the classic study in this area is by Marsh Ray (1961: 132-40). Ray begins his analysis by noting that physical withdrawal from opiates can be secured in a short period and without mental impairment. From this it is concluded that relapse behavior is a phenomenon in need of social-psychological explanation. The alleged explanation lies in the interaction between the deviant and society.

Society, it is proposed, supplies 'secondary status' characteristics to the drug addict (e.g., criminal, mental abnormal, bum, etc.). These characteristics become the 'medium of exchange' between the addict



and the non-addict world. During his period of abstinence, the addict is seeking ratification of his non-addict role. But society is skeptical of this new role and during the exchange transactions withholds the needed rewards. The result of this interaction process, Ray concludes, is the eventual surrender of the abstainer to his original role of addict. Thus relapse is seen as a product of society's insistence on imposing punitive labels. Substituting Tannenbaum's terminology for Ray's: it is society's persistence in dramatizing the role of evil that insures the perpetuation of evil's existence. And like Tannenbaum, it seems that Ray would conclude that "the way out is through a refusal to dramatize evil" (1938: 20). The implication is, obviously, that our laws should be changed.

#### THE LAW AND THE ADDICT

North America is given to polarized attitudes and the situation with respect to opiate addiction is no exception. For every Timothy Leary who recommends the legalization of drug distribution there is a Mayor Daley who suggests that all pushers should be shot. Sociology, in spite of its recent infatuation with the labelling perspective, has often mirrored this schizophrenic mentality.

Schur: It is, in fact, quite possible that a nonpunitive approach, such as the British have taken, increases the likelihood of enlisting the cooperation of addicts at cure (1965: 155).

Yablonsky: I personally and professionally feel that it is totally absurd to give an addict drugs. It is like giving a person dying of cancer more cancer! (1967: 376).

Where feeling and belief provoke this amount of confusion, known





facts may provide some much-needed clarification. This section will consider what facts are known with reference to British legislation in the area of opiate addiction.

Before reviewing the facts of the British experience, however, it will be valuable to consider just exactly what theoretical assumptions, in addition to policy recommendations, are involved in this discussion. The theoretical formulation under consideration is, of course, the labelling hypothesis. This hypothesis comes, with reference to opiate addiction, in two forms.

(I) The Strong Version: This version of the labelling hypothesis alleges that if opiates and/or their synthetic substitutes were distributed through legal channels, it would reduce the problems of opiate abuse. This would occur in two ways. First, legal access to addicting drugs at minimal prices would eliminate the black market's clientele--thus leaving control of drugs completely in government hands. Second, the treatment of addicts as patients rather than as criminals would reduce stigmatization, thereby encouraging ex-addicts to fulfill the requirements of an abstinence role in society. It is assumed that by restoring the addict's legitimacy, and by ratifying his role as abstainer, that it would be possible to prevent relapse.

(II) The Weak Version: A diluted version of the labelling thesis argues that legal distribution of drugs derived from or equivalent to opiates would at least prevent the problems of opiate abuse from expanding. Here it is again argued



that legalized distribution would eliminate the economic base of the black market. But more important, it is urged that by reducing the stigma of addiction we could confine the problem to its primary forms. There would be no need for the addict to engage in secondary crimes (e.g., theft of money and/or drugs) to support his habit and the addict's self-concept would escape dramatization. Such a situation would reputedly allow the addict to return to a useful, if not completely normal, role in society. The drug subculture would vanish, its reinforcement no longer needed by the addict, the black market would collapse, legal provision of drugs having replaced it, and addiction would therefore at least not increase. While not promising cure, this version of the labelling hypothesis does promise containment.

The essential difference between these two applications of the labelling hypothesis is that the first expects to eliminate opiate abuse, while the second hopes only to stall the expansion of the problem in its primary form and reduce its secondary effects. The testable similarity between the two versions of the labelling story is, however, the prediction of both that the incidence of addiction will not increase. A look at the British experience will provide some of the facts needed to carry out this test.

Great Britain has followed an approach to the opiate problem that is distinctively different from the American example. In Britain, addiction is treated almost entirely as a medical matter. Movement in this direction began legislatively with the original Dangerous Drugs Act





in 1920. Following an advisory report in 1926, this act was rewritten and supplementary regulations added. The result was a drug policy giving treatment responsibility to medical practitioners--allowing physicians legally to supply narcotics to addicts. Under the plan, the physician retains final responsibility in determining what constitutes proper medical treatment of addicts. Doctors are not legally required to register the addicts they treat, but they are 'requested' to inform the Home Office and Special Ministry of Health inspectors of those addicts receiving regular attention. It is thus believed that the Office's records contain data on most of Britain's addicted persons (Schur, 1965: 152-54).

Until the early 1960's, the British drug problem remained as in Schur's description: "remarkably benign." By 1959 there were still only 68 known heroin addicts of whom 21 were thought to be therapeutic addicts. But the decade of the sixties brought something of an epidemic to the British drug scene. In December of 1966 general practitioners reported 223 addicts, and by January of 1968 the figure was about 250. Ten months later, the number of reported addicts had increased to 899. Using the next few months as a base for projections, authorities expected that 800 new addicts would be reported in 1969--thus reflecting a near one hundred per cent increase in cases of reported addiction (Lapping, 1968: 521-22). Obviously, not all cases of addiction are reported and some sources indicate that the slippage between official and actual addiction figures is quite large. Thus one researcher indicates that the number of drug addicts increased from 400 in 1958 to 1,349 in 1966 (Jones, 1968: 109-14). Regardless of these



discrepancies, however, the uniform conclusion seems to be that opiate addiction is rapidly increasing in England.

The outcome of the British experience suggests support for neither the strong nor the weak version of the labelling hypothesis. Opiate addiction in Great Britain is increasing. The results of the British experience should not surprise us. We have argued earlier that it is the positive association formed between the reduction of withdrawal symptoms and the acquisition of opiates that is the crucial determinant of addiction. Attempts to alter radically the nature of addict behavior must deal with this central factor. Public policies concerning opiate addiction based solely on the frailties of the cognitive bias seem destined to disappointment.

#### THE BEHAVIORIST ALTERNATIVE

There recently has emerged a behaviorist alternative to cognitively based recommendations concerning opiate addiction. This alternative emerges from a background of efforts to change behaviors using the techniques of aversive conditioning. This stylistic approach to the problems of behavior modification has implications for the treatment of opiate addiction.

Aversive conditioning, in the case of drug addiction, attempts to undermine the positive association formed between the use of opiates and the reduction of withdrawal distress. This is accomplished by making the injection of narcotics noxious and unpleasant instead of gratifying. Robert Liberman (1968: 229-31) has completed a recent series of experiments using this technique.



Liberman performed his experiments on two narcotic addicts who had been withdrawn for several months. The patients first received an injection inducing nausea, cold sweats and dizziness. Five to ten minutes later the patients were instructed to "mainline" an injection of morphine. Approximately one minute later the aversive stimuli took effect. These unpleasant effects were sufficient to override the 'rush' or euphoric effects supplied by the 'fix' and they persisted for more than thirty minutes.

After about three sessions, the patient already has established a strong, conditioned aversive response to the 'fix' situation and after the first week a choice situation is introduced. In the choice situation, the patient enters the treatment room and can choose between the syringe with morphine and more socially appropriate items such as coffee, soft drinks, cigarettes and candy. If he chooses the food or cigarettes, he spends thirty minutes eating, smoking and conversing amiably with myself and the nurse. The rationale for the choice situation is that it provides positive reinforcement for a socially appropriate response and avoidance of the anxiety that accompanies the conditioning trail. It also allows the patient to take active control of his treatment instead of being in a passive position. The free choice situation is interspersed among the conditioning trails in a random manner for a total of six times (1968: 229-30).

Of the two patients conditioned in the manner described, one relapsed, while the other has remained abstinent, at least to the time when Liberman submitted his research paper--a period of one year.

Obviously, it is much too early to form any definite conclusions about the utility of behavior modification techniques in relation to the treatment of opiate addiction. New therapies are all too often accompanied by an overabundance of unwarranted optimism. And it is further necessary to acknowledge the low success of aversive conditioning in the treatment of alcoholism--a situation with some parallels to the drug problem. The difficulty in both treatment situations is keeping the





client faithful to his therapy. It seems that all too often under the conditions of voluntary choice of aversion therapy or the drug, the once-addict will choose the drug. Despite these difficulties in treatment, the results of the behaviorists' experiments should be of interest to both sociologists and other practitioners interested in altering sustained-opiate-directed behavior.

### CONCLUSIONS

Our excursion into the world of opiate addiction has focussed on an evaluation of two explanations: the mentalist and the behaviorist. We have found the first of these explanations to be characterized by a cognitive bias. Its description of the addiction process seems superfluous, obscuring many of the specific details essential to that process, and its extension into the realms of policy formation seems misguided. In contrast, we have found that the second explanation pays the necessary attention to the specific behavioral details essential to the addiction process. A translation of the principles used in this second explanation into a procedure for behavior modification has provided an alternative for the treatment of opiate addiction.

In the course of our discussion we have indicated a lack of patience with the notion of self-concept--at least as it is applied to the explanation of opiate addiction. One source of this impatience is the lack of consistency with which this 'theoretical tool' is applied to the drug addict; there are examples. Lindesmith (1968) has described the drug addict as if he were caught in the most sadistic of "self-degradation ceremonies." In contrast, Finestone (1964) has described



the "cat" and created an image of stultifying "self-aggrandizement." Yablonsky (1967), on the other hand, has provided a picture of the addict in which we are to observe a process of near "self-elimination." Finally, Latendresse (1968) has supplied a provocative finale, comparing the addict to the chronic masturbator and concluding that neither have the capacity for "self-acknowledgment." The reader should prepare for a confusing journey.

Lindesmith: Prior to addiction, the addict generally shares the negative attitudes of the society towards junkies or dope fiends. When he himself becomes addicted, he necessarily applies these attitudes to himself and his conduct. The realization that one has become an addict is not pleasant; it is a self-conception that is impressed upon the user when he is trapped by the drug (1968: 137, emphasis added).

Finestone: The "cool cat" exemplifies all. . . elements in proper balance. He demonstrates his ability to "play it cool" in his unruffled manner of dealing with outsiders such as the police, and in the self-assurance with which he confronts emergencies in the society of "cats." Moreover, the "cat" feels himself to be any man's equal. He is convinced that he can go anywhere and mingle easily with anyone (1964: 285, emphasis added).

Yablonsky: The criminal addict, when he is acting out his plight, tends toward anarchy. He lives outside the law most of the time. He even rejects the confines of a coherent ego; for to accept a "self" is, in some measure, to accept society's norms and recognize boundaries for behavior (1967: 76, emphasis added).

Latendresse: It appears that both the chronic masturbator and the addict not only have a lack of capacity for self acknowledgment but a specific readiness for a certain state of blurring consciousness which relates to the fusion of body-mind (1968: 20, emphasis added).

We have seen visions of the addict's self-concept as negative, positive, rejected, and finally incapacitated. The means of resolving the contradictions between the various assertions is not clear. We could





ask the addict himself, but this would involve all the problems of equating what people think with what they say (see Chapter Two, Hypothesis III). At least with respect to opiate addiction, we would suggest an alternative solution: rather than attending to words, the sociologists would do better to attend to behaviors.

We began this discussion with two goals. First, we sought a resolution, with specific reference to the problem of opiate addiction, to the ongoing dispute between the behaviorists and the mentalists. In this case, we have concluded that the behaviorist explanation is of more utility. Our second goal was to convince sociologists that the assumption of a cognitive basis for human activity must be re-examined in each of the wide range of behavioral situations. We do not conclude that the behaviorist explanation is definitive for all forms of human activity. Further, we assume that man's cognitive capacities must influence some varieties of behavior. Thus we must leave the reader with the same empirical question formulated at the beginning of this discussion: "Just exactly what varieties of behavior are mediated by precisely how much cognition?" Sociology will not benefit from a continuing reluctance to acknowledge these questions.



## CHAPTER FOUR

### IN SEARCH OF ESSENCE: THE METHODOLOGICAL IMPLICATIONS OF AN INTERACTIONIST PERSPECTIVE IN DEVIANCE

The search for essence, like the pursuit of meaning, has often preoccupied advocates of symbolic interactionism. The growing popularity of the interactionist thoughtway has influenced a translation of this preoccupation into the analysis of deviance. The methodological accompaniments of this theoretical translation have been an implementation of the techniques of analytic induction and a recent interest in the field of ethno-methodology. It will be argued in the following discussion that these methodological styles are variously conducive to naïve, as well as at times, promiscuous conceptualization.

#### "NATURALISM" AND THE STUDY OF DEVIANCE

The search for essence involves an attempt to avoid philosophical preconceptions. That a search for essence must by definition carry its own preconceptions is for the moment ignored. Instead, the emphasis is placed on the effort to elude such intermediaries between the observer and his subject. The epistemological device invoked for the achievement of this goal is "naturalism."

Naturalism, as the very term implies, is the philosophical view that strives to remain true to the nature of the phenomenon under study or scrutiny. . . . So conceived, naturalism stands against all forms of philosophical generalization. Its loyalty is to the world with whatever measure of variety or universality happens to inhere in it.



Naturalism does not and cannot commit itself to eternal preconceptions regarding the nature of phenomena (Matza, 1969: 5).

Acting under the instructions of naturalism, the deviance analyst is advised to enter directly his subjects' world of "experience."

. . . to do a lot of free exploration in the area, getting close to the people involved in it, seeing it in a variety of situations they meet, noting their problems and observing how they handle them, being party to their conversations, and watching their life as it flows along (Blumer, 1969: 37).

There is a promise associated with the observational procedure described, and it suggests that first-hand experience brings knowledge.<sup>1</sup>

The character of this knowledge is thought to be of a special sort. It is alleged to contain the very essence of existence. Not a facsimile, no simple approximation--we are promised the substance of being. In short, we are promised what is.

To locate or assert an essential feature of phenomena is a basic part of naturalist analysis, as basic perhaps as posing a relationship between two variables is in more conventional sociological analysis. For the naturalist, the location of essential features is crucial because it is an attempt to cogently assert what the phenomenon is. The assertion of essential features is nothing less than an analytic summary of the phenomenon (Matza, 1969: 27).

As indicated above, there is a promise associated with the procedure described. This promise warrants an evaluation.

An evaluation of "naturalism" can proceed along two avenues: one involves the use of simple logic, the other considers pragmatic utility. This section will consider some logical implications of the naturalist viewpoint; succeeding sections will analyze in detail the logical consistency and the pragmatic utility of the naturalist viewpoint, as embodied in the method of analytic induction and in ethno-methodology.





Naturalism is distinguished as paying proper respect to the subjectivity of being human (Matza, 1969: 7). The focus is placed on man's subjective experience of empirical reality.

Naturalism must choose the subjective view, and consequently it must combine the scientific method with the distinctive tools of humanism--experience, intuition, and empathy (Matza, 1969: 8).

Naturalism here operates as a lever to pry open a Pandora's box containing all the distortions of subjective experience. It now becomes possible for naturalism to tell us that the conceptions of a deviance analyst determine what he will find when researching his subjects.<sup>2</sup> Using this argument as its vehicle, naturalism implies that all conventional explanations of deviance are invalid. But this argument, taken to its logical extreme, also tells us that any thoughtway must suffer a similar fate, naturalism included--good intentions notwithstanding. To avoid this entrapment, and to halt the unending regression familiar to relativistic styles of thought, naturalism seeks to provide a means for slamming the lid back on our metaphorical box. And it is here that analytic induction and ethno-methodology are promoted as techniques for establishing, on virgin turf, new explanations of deviance. The first method promises purity via the quest for universals; the second method relies on the search for invariant properties. Enervated by their alleged chastity, both methods engage in a fervent search for essence.

#### ANALYTIC INDUCTION AND THE "QUEST FOR UNIVERSALS"

##### Znaniecki and the Method of Sociology:

The roots of analytic induction and the reasons for its existence have been variously traced (Lindesmith, 1953: 492-93). However, the



forceful introduction of this methodology into the field of sociology is most directly linked with early work done in Chicago by Thomas and Znaniecki.

In fact, William I. Thomas was probably the first who based sociological research entirely on the analysis of particular cases, using several different instances for every generalization. He developed this method chiefly in his lectures from 1905 to 1915 and so influenced many young sociologists. In his Source Book in Social Origins the method was already partly applied. We used it together on a large scale in the Polish Peasant. The disciples of Thomas have since spread it pretty widely (Znaniecki, 1934: 237-38).

It is, of course, no surprise that W. I. Thomas was also one of the first proponents of symbolic interactionism.

Although Thomas seems to have sparked the emergence of analytic induction in sociology, the task of making the method explicit fell to his co-author, Florian Znaniecki. Znaniecki pursued his task faithfully; soon analytic induction appeared, its formation complete, as The Method of Sociology.

The Method was seen by Znaniecki as a replacement for the more conventional reliance on enumerative, or as it is more often known, statistical induction. A summarization of what distinguishes the two approaches would appear as follows: enumerative induction engages in a search for similarity, while analytic induction involves a search for essentiality. At least Znaniecki saw this distinction as being more than a matter of semantics.

It may be said that analytic induction ends where enumerative induction begins; and if well conducted, leaves no real and soluble problems for the latter. With such a radical difference in logical problematization, the logical procedure should naturally differ widely. While both forms of induction tend to reach general and abstract truths concerning particular and concrete data, enumerative induction





abstracts by generalizing, whereas analytic induction generalizes by abstracting. The former looks in many cases for characters that are similar and abstracts them conceptually because of their generality, presuming that they must be essential to each particular case; the latter abstracts from the given concrete case characters that are essential to it and generalizes them, presuming that insofar as essential, they must be similar in many cases (1934: 250-51).

If the above discussion is to reflect more than a semantic difference, then the method must supply us with a specific technique for initially identifying the "essential characters" of a phenomenon. A challenge has been issued.

Attempts to answer this challenge have not been convincing. In fact, efforts to confront this central question surrounding the use of analytic induction seem often to lead in circles.

Once we have selected a system as object-matter of study, we know that everything that characterizes it belongs to it, and goes on within it is relatively essential as compared with all the accidental data which, while accompanying its actual existence, are not included or are explicitly excluded from it as irrelevant (1934: 252).

If the above is true, then it would seem to be so by virtue of redundancy. In telling us that the essential features of a social system are defined by their inclusion in that system, Znaniecki fails to provide any independent means of knowing that which is essential. Instead, Znaniecki seems to be suggesting that we can "know" essentially by definitional fiat. Knowledge by means of definition and faith rather than by proposition and independent measure suggests the liability of tautology. Thus the challenge to provide a valid technique for initially determining what is essential to a datum is still left unanswered.

But tautology can provide reassurance, and encouraged by its support, Znaniecki proposes a second and most important step in



analytic induction.

. . . when a particular concrete case is being analyzed as typical or eidetic, we assume that those traits which are essential to it, which determine what it is, are common to and distinctive of all the cases of a class (1934: 252).

As we shall see, this step marks a decisive turn in the "quest for universals."

The third step in analytic induction involves the attempt to establish the universality of the essential features hypothesized. This version of hypothesis testing is directed by a search for exceptions--or negative cases, as they are sometimes known. It is assumed that ". . . testing every such hypothesis by the analysis of exceptions will prove the most fruitful scientific procedure" (1934: 306). We find this assumption to be at least unsafe, and more likely inaccurate.

There is an outstanding danger associated with the requirement that there be no exceptions to a hypothesis. The danger is that the quest for universal truths will be productive of promiscuous conceptualization. In other words, the danger is that the hypothesis will be made invulnerable by means of concepts so broad in scope that any empirical event may be contained within their boundaries. A hypothesis that cannot be falsified must be definition be universal. The liability of such defensive strategies of theory building is impotency. The resulting theories simply cannot predict.

But Znaniecki neglects these difficulties and completes his procedural description with the provision of a fourth and final step. This step is in concert with more conventional methodologies. It instructs that we organize our essential characters into related classes, ". . . based on the functions the respective characters play in determining





them" (1934: 260).

There is yet another promise accompanying the completion of this final stage in our investigation. And this additional promise of analytic induction includes a further incentive than that already provided for our initial acceptance of the naturalist thoughtway. We are promised not only what is, but also the control of what can be. Such a promise is attractive to those of us who desire change in the social processes that surround us.

. . . if the generic conditions or causes of these processes can be determined by analytic induction, by modifying these conditions or causes, the frequency of the processes themselves can be influenced (1934: 329).

The aspiration towards "doing good" is here abetted with a method. However, following sections will demonstrate that expectations have been unjustly inflated, and that the promise of analytic induction is in fact false.

#### Analytic Induction as a Method of Causal Analysis:

Our assertion that the promise of analytic induction is false requires support. An analysis of the logical structure of the method can provide this support. But before embarking on this analysis, an important point must be made: the original statement of analytic induction is inconsistent with the method in practice. In this section we will consider the method as set out in Znaniecki's statement; later sections will consider the method in practice.

There is a structural impairment that prevents analytic induction from being a logically adequate method of causal analysis. This impairment is, quite simply, that the method provides consideration only of





necessary, and not sufficient, conditions for the occurrence of the phenomenon under study.

It is easy to show that the method of analytic induction as described gives only the necessary and not the sufficient conditions for the phenomenon to be explained. The method calls for studying only those cases in which the phenomenon occurs, and not the cases in which it does not occur. To study cases in which the phenomenon does not occur would involve us in enumerative induction, the comparative method, for which Znaniecki would substitute the method of analytic induction (Robinson, 1951: 814).

In order for the practitioners of analytic induction to specify both necessary and sufficient conditions for the occurrence of a phenomenon, he must demonstrate that the phenomenon never fails to occur in the presence of these conditions. If the phenomenon did in fact fail to appear in instances where the conditions were present, then these conditions would not, by definition, be "sufficient cause." Znaniecki's insistence that the method's advocates look only at cases of the phenomenon's occurrence negates the possibility of ever locating both necessary and sufficient causes.

This argument may further help to explain analytic induction's predictive impotency.

#### Analytic Induction as a Method of Prediction:

There are by now at least four reasons for the method's failure to aid in the art of prediction.

- (1) The first explanation flows from the argument presented in the previous section.

This argument shows why the method of analytic induction as described by Sutherland and Cressey cannot enable us to predict. It cannot because it gives us only the necessary



and not the sufficient conditions for the phenomenon to be explained. Only if we know that the phenomenon never fails to occur in the presence of the conditions C, . . . can we predict the occurrence of the phenomenon from C (Robinson, 1951: 815).

But this is only a partial explanation for the method's impotence.

- (2) A second source of analytic induction's failure to predict is found in an argument, made earlier, that the method is conducive to promiscuous conceptualization. Here it was emphasized that the method's quest for essence, and the requirement of universality, are productive of a defensive strategy of conceptualization. To avoid exceptions, or negative cases, concepts may be constructed so broad in scope that they are invulnerable to exception. The other side of this coin is that the concepts will be so general as to make prediction impossible. Invulnerability is bought at the price of prediction.
- (3) The correlates of an invulnerable scheme of conceptualization, tautology and post factum explanation, provide a third reason for the method's failure. Inevitably, concepts constructed on a non-specific base find themselves overlapping one another, and more often than not, becoming identical in content. The practical import of conceptual isomorphism is obviously a failure to obtain independent variables. The failure of analytic induction to elicit independent variables, and its relation to the method's inability to predict, is described by Turner.





The. . . general reason for lack of empirical prediction is that the alleged preconditions or essential causes of the phenomenon under examination cannot be fully specified apart from observation of the condition they are supposed to produce. In any situation in which variable "A" is said to cause variable "B," "A" is of no value as a predictor of "B" unless we establish the existence of "A" apart from the observation of "B" (1953: 606).

The situation described is obviously one of tautology. And while the use of tautology may provide consolation to those who enjoy fixed arguments, it provides no ammunition for those who would want to predict. The method of analytic induction is conducive to exercises in ad hoc explanation; and like the Monday morning quarterback, its practical utility is limited.

- (4) A fourth source of analytic induction's failure to predict reflects the naivety of the method. Given our inability to ever know perfectly the "true" nature of empirical reality, we are skeptical of "quests" for universal relationships among independent concepts.

. . . our argument must be to ask why the search for universals does not carry us beyond formulating a definition and indicating its logical corollaries, and why it fails to provide empirical prediction. The answer may be that there are no universal, uniform relations to be found except those which constitute logical corollaries of conceptual definition. The positing of operationally independent causal variables, empirically assessible prior to the existence of the postulated effect, always seems to result in relationships of statistical probability rather than absolute determination (Turner, 1952: 609).

It can be added that analytic induction utilizes a rather simplistic viewpoint in assuming that only a few variables



account for all the variance attributed to a phenomenon.

Devotees of multi-variate analysis would obviously take exception to such an assumption. In just this vein, Turner (1953: 609) finds analytic induction "rather ill-equipped to cope" with the fact of multiple determination.

We have found analytic induction to be a method lacking predictive utility. This verdict has been reached after an assessment of the logical foundations of the method--vis-a-vis its statement by Znaniecki.

We now turn to an analysis of the method in practice.

#### ANALYTIC INDUCTION IN PRACTICE:

#### AN EXERCISE IN NAÏVE EMPIRICISM

Early in our discussion, W.I. Thomas was given credit for having anticipated the method of analytic induction. It was Thomas who most dramatically called attention to a need for intensive investigation of "particular cases." In deference to this emphasis, analytic induction has also been known as the limited case method. This section of our discussion will argue that in practice analytic induction is distinguished from more ordinary statistical induction only by its failure to produce empirically useful concepts and by its emphasis on particular cases. In this sense, the practice of analytic induction may be characterized as an exercise in naïve empiricism.

#### Distinction without Difference

To this point, our discussion has produced two central characteristics thought to distinguish analytic induction from more conventional statistical induction. The first of these is the search for negative



cases in the verificational quest for universals. The second is the edict that the method's advocates examine only those cases in which the phenomenon under study occurs. It is to be shown, however, that in practice neither of these characteristics are reflective of a qualitative methodological difference.

- (1) Dedication in the search for negative cases is among the qualities admired by followers of Znaniecki's method. It is thought that this persistent search distinguishes those who aspire to the method. However, in practice, advocates of statistical induction persist in a similar search.

. . . this insistence upon analysis of deviant cases is not logically different from the similar insistence of the sophisticated practitioner of enumerative induction. The practitioner of enumerative induction phrases it differently. He says that he looks for a new variable correlated with his residuals, so as to include it in a new multivariate analysis; but it amounts to the same thing. The point is that he keeps modifying his hypothesis to account for the failures of his original relation to predict infallibly (Robinson, 1951: 813-14).

The fact that the practitioner of statistical induction realizes that he will only approach, and never reach, a state of infallibility (i.e., the discovery of universal relationships) indicates a difference of degree, and not kind, from the practitioner of analytic induction. However, this difference of degree should not be de-emphasized. It is the ability to separate aspiration from accomplishment that enables the advocate of statistical induction to resist the construction of infallible, but predictively useless, concepts.





- (2) Znaniecki was entrapped in a logic-bound straight-jacket. He thus found himself urging the researcher to avoid analyzing situations where the phenomenon under investigation did not occur. To operate otherwise was to recommend comparison--the basis of statistical induction. But, as we have seen, avoidance of the comparative method yields explanations built around necessary, and not sufficient, causes.

Both Lindesmith and Cressey have sensed this inadequacy of analytic induction as stated, and neither has applied it in the form in which it is stated. Lindesmith made a systematic study of non-addicts to determine whether addiction ever failed to occur when his conditions were present. . . . Cressey. . . assumed that before their defection his violators were representative of all non-violators. . . . His assumption is open to question and should be tested, but his intention to include non-violators as well as violators is unmistakable (Robinson, 1951: 815-16).

The import of practicing analytic induction in the manner described is the simple fact that it involves the use of the comparative method. And this, of course, brings us back to statistical induction--the method Znaniecki intended to displace.

With both methods now looking at the same variety of data, we are returned to only a single difference of degree. This difference, again, is the requirement that there be no exceptions to the final hypothesis among the particular cases studied. In working terms, the requirement is that the phenomenon must always occur when the specified conditions are present, and never occur when the conditions are absent. Once more, it is argued that this naïve belief in the prospects of achieving



universality, through excessive attention to a limited number of cases, is the method's undoing.

This accusation requires support. An analysis of Cressey's study of embezzlement will provide this support.

#### The Non-Shareable Problem: Cressey's Theory of Embezzlement

The method stands accused of promiscuous conceptualization. The source of the problem is, we have argued, the quest for universality. In other words, we are suggesting that the requirement of universality results in concepts so general that virtually any empirical event may be contained within their boundaries. Supportive evidence is now required. It will consist of excerpts from Cressey's own description of the process whereby he developed the central concept of his embezzlement thesis.

Cressey arrived at his central concept after testing four hypotheses. The first three hypotheses were rejected after the discovery of negative cases. The fourth hypothesis withstood the threat of exception for fifteen years; an insight into the durability of this hypothesis can be gained by analyzing the events preceding its formation.

Cressey's first hypothesis was derived from Sutherland's writings on white collar crime. As one would expect, this hypothesis centers around definitional attitudes learned by the actor in interaction with his associates.

The initial hypothesis, which was abandoned almost immediately, was that positions of financial trust are violated when the incumbent has learned in connection with the business or profession in which he is employed that some forms of trust violations are merely 'technical violations' and are not really 'illegal' or 'wrong,' and, on the





negative side, that they are not violated if this kind of definition of the behavior has not been learned (1950: 741, emphasis added).

Although this hypothesis is definitely not specific enough to be useful in prediction (i.e., what are the empirical referents of the various definitions described?), it will be shown that each of the succeeding hypotheses are progressively less specific. Each step in this process is provoked by the appearance of negative cases. In this first instance, exceptions to the proposed hypothesis consisted of cases in which trust violators defined their acts as more than "technical violations."

His first hypothesis having failed, Cressey quickly set about formulating a second hypothesis in slightly more general terms.

. . . a second hypothesis, which included some of the 'multiple factor' ideas of gambling and family emergencies, as well as the potential trust violators' attitudes toward them, was formulated. . . . The formulation was that positions of trust are violated when the incumbent structures a real or supposed need for extra funds or extended use of property as an "emergency" which cannot be met by legal means, and that if such an emergency does not take place trust violation will not occur (1950: 741, emphasis added).

This second hypothesis is inclusive of not only the actor's definitions of his surrounding situation, but also contains reference to the situation itself (e.g., gambling and family emergencies). Exceptions to this second hypothesis involved cases where no emergency was found to exist.

Cressey's third hypothesis was formulated by adding a psychological component and extending the situational reference.

The next revision shifted the emphasis from emergency to psychological isolation, stating that persons become trust violators when they conceive of themselves as having



incurred financial obligations which are considered as non-socially-sanctionable and which, consequently, must be satisfied by a private or secret means. Negatively, if such non-shareable obligations are not present, trust violation will not occur (1950: 741, emphasis added).

The shift in emphasis from emergency situations to financial obligations of a non-shareable nature marks a sizeable step backwards into conceptual obscurity. While definitional attitudes towards emergency situations are difficult enough to decipher, a non-shareable financial obligation is nearly impossible to pin down. By definition, one cannot directly know another person's non-shareable financial obligations. Such things can only be identified in retrospective disclosures after the situation has passed. The disutility of ex post facto explanations is known best by those who have already been victimized. To explain backwards only obscures our problems; solution requires, among other things, the ability to predict.

It should be added that Cressey's substitution of "financial obligation" for "emergency" creates a considerably broadened conceptual scheme. In spite of the widened scope inherent in Cressey's third hypothesis, a few exceptions were found. These negative cases were characterized by the absence of financial obligation.

The fourth version of the embezzlement hypothesis finally brought goal-attainment: universality--at least in terms of the original sample.<sup>3</sup> But this victory was not gained without again broadening the theory's central concept.

Again the hypothesis was reformulated, emphasizing this time not financial obligations which were considered as non-socially-sanctionable and hence as non-shareable, but non-shareable problems of that nature (1950: 742).





The conceptualization promiscuous, the empirical referents obscure, the formulation could scarcely be other than universal--the theory has been rendered invulnerable. Everyman has financial problems, and the requirement that they be of an unshareable sort insures that they will only be identified in ex post facto explanations of past behaviors. We all justify, and Cressey's theory may merely provide convenient means for doing so. It is a perfect explanation, and as such renders its subject matter perfectly unpredictable.

All of this illustrates the perils to be encountered when following a path of universal expectations. We have seen evidence that the path leads to conceptual promiscuity. The moral to this story speaks of the futile consequences attendant to naïve empiricism. To count, classify, and correlate are preferences within a scientific style of thought. Attention to particular cases can be both rewarding and instructive. But to demand in all this that universality prevail is to mix aspiration with achievement. We would do better to moderate our expectations.

#### THE NEO-CHICAGOANS' NEW CLOTHES: ETHNO-METHODOLOGY AND THE SEARCH FOR INVARIANT PROPERTIES

The emergence of a "new deviance analysis" (Schur, 1969) has been accompanied by a variation in methodological style, if not by a moderation in expectations. This latest in stylistic methodologies travels under the school name of ethno-methodology. The jumping off point for ethno-methodology, as for the rest of sociology, is the existence of social order. However, ethno-methodology allegedly offers an unique





viewpoint on the order of everyday life. Instead of assuming social order, we are to challenge its existence. It is assumed that this confrontation will yield knowledge. Garfinkel's "experiments" are regarded as the prototype for this variety of research.

Garfinkel's strategy is to begin with a situation viewed as "normal" and then systematically attempt to create "trouble," confusion, or chaos. The procedures producing chaos would suggest obversely the elements of stable order (Cicourel, 1964: 168).

The knowledge-giving potential of this unusual experimental approach is as yet unmeasured.

There are additional assumptions that are thought to distinguish ethno-methodology. The most basic of these premises are expressed by Cicourel.

There are "rules" and properties. . . which operate to structure what the sociologist ordinarily calls "norms." These "rules" and properties are invariant to the actual content and type of "norms" which govern social action in particular situations. The study of these "rules" and properties provides an experimental foundation for the measurement of meaning structures basic to all sociological events (1964: 171).

Cicourel seems to be making three basic points:

- (1) that "invariant rules" underlie the "normative social order,"
- (2) that "meaning structures" underlie "social action," and
- (3) that all four of these concepts are interrelated.

The above translation would seem to take on the character of a massive tautology. "Invariant rules," "normative social order," "meaning structures," and "social action" all appear to be concepts made from the same piece of cloth. "Social action" is made up of "meaning structures" which are the "normative social order," all of which are "ruleful" and thereby describable in the first place. The only unique



aspect of Cicourel's discussion is the ascription of invariance to these rules. This ascription of invariance may be attributed, once again, to naivety. If we have learned nothing else in sociology, we have at least learned that little of substantive merit is true all of the time. Yet once again we are caught in the trap of having to prove this to the neo-Chicagoans.

One final item of terminology must be introduced before we turn our discussion to the role of ethno-methodology in deviance analysis. This final term, "typification," is derived from the work of Alfred Schutz. Cicourel provides an illustration of the term's usage and draws a quotation from Schutz to make its meaning explicit.

Socially distributed knowledge taken for granted in everyday communication is exchanged within a context whereby the actor typifies both his own and the other's behavior. Typical social roles and typical expectations are assumed in the exchange of socially distributed and socially approved knowledge. "Socially approved knowledge consists, thus, of a set of recipes designed to help each member of the group to define his situation in the reality of everyday life in a typical way" (Cicourel, 1964: 216).

Typification thus appears to be one of the invariant properties allegedly associated with the normative order of everyday life. But again we seem to be caught up in a tautology. The concepts of typification and normative order seem inherently synonymous. One may conclude that the requirement of invariance, like the insistence on universality, yields conceptual promiscuity. Worse yet, usage of the term typification seems to risk not only redundancy--but also utter banality. Nevertheless, this concept, as well as those preceding, have been translated into the study of deviance.





### The Radicalization of 'Deviance'

In the field of deviance, ethno-methodology has been a fellow-traveller with the labelling perspective. It can be argued that the labelling perspective is in itself a tautological thoughtway--that is, it can be argued that the labelling perspective engages itself in relating concepts that are not independent of one another. The labelling theorists themselves, Zimmerman taken as an example, argue quite explicitly against conventional attempts to distinguish independent variables for the purpose of studying deviance.

. . . the distinction between primary and secondary deviance, or between rule-violation and deviance, must be reconsidered. . . the contrast cannot be between a rule-infracton, on the one hand, and the societal-reaction labelling process, on the other, the two being conceived as independent events (Zimmerman, 1969: 14).

Zimmerman goes on to suggest that both rule violation and deviance are part of a larger interaction process within which variables cannot be isolated. It is argued that focussing on rule violation is a mistake in emphasis.

To employ rule-violation as a sufficient criterion of deviance presupposes what the interactional conception of role makes problematic, the stable meaning of rules across interaction situations. . . . What is to be accounted for, then, is the interaction process through which the identification of deviance is accomplished. This is not a subsidiary question, it is the question (Zimmerman, 1969: 15).

What is being urged, of course, is an ethno-methodological approach to deviance analysis. Zimmerman feels that such an approach would radicalize the labelling perspective in deviance. Perhaps as accurate a conclusion might be that both thoughtways have been engaging in the same tautological thought pattern all along--not a surprising conclusion when one remembers that both of these explainways have their



origin in the Meadian tradition of symbolic interactionism. But more of this later; now we must turn to an empirical test of a set of premises thought to be basic to both ethno-methodology and labelling theory.

### The Ethno-methodology of Juvenile Justice

Aaron Cicourel, in his recent book The Social Organization of Juvenile Justice, has attempted to provide an ethno-methodological explanation of the process by which juvenile behavior is "transformed" into juvenile delinquency. Using a series of assumptions similar to those urged by Zimmerman, Cicourel tells us that official records of delinquency are a function of an interaction process involving the juvenile and agents of social control. This interaction process is said to be guided by a "common meaning structure"--conventional or "natural" explanations of delinquency--that determine the ascription of the delinquent label to accessible juveniles. The implication of this thesis is that our "natural" explanations of delinquency, rather than the juvenile behaviors themselves, are the central determinants of who is to receive the label of "delinquent."

This viewpoint contains at least two potential hypotheses, both seemingly amenable to empirical test. The first of these is a thesis proposing that sociological theories of delinquency are a simple reflection of the natural or "empathetic" explanations constructed by the lay public.

The articulation of motivational and organizational variables in sociological theories appears quite 'natural.' The fact that this union almost always implied it was the children of slum dwellers who are most likely to become delinquent also seems to follow quite 'naturally.' The





development of welfare legislation, settlement houses, the juvenile court, and sociological theories attributing delinquency to youth from poverty-ridden, disorganized neighborhoods with unstable homes and gangs with nothing 'constructive' to do, all in a context of rapid industrialization and urbanization, seems 'natural.' The use of the term 'natural' is intended to underscore the congruence between sociological and lay theories of delinquency (Cicourel, 1968: 25).

Cicourel's thesis is not without foundation, and in fact seems quite plausible. Among others who have proposed similar hypotheses--in perhaps greater detail--are Leifer (1964), Matza (1964), and Nettler (1970b). One version of the hypothesis could be phrased as follows: popular explanations of bad behaviors are most often constructed on the causal base of bad environmental conditions, whereas explanations of good behaviors are usually to be found in references to good intentions and/or purpose. Nettler has identified a possible moral background to this variety of thought.

In the current use of causes, evil acts must have evil roots. We should find it uncomfortable, if not immoral, to assign bad effects to good causes, and vice versa (Nettler, 1970b: 162).

A thorough empirical test of the proposed hypothesis is as yet unavailable. At this point we can only conclude that the proposition has inferential support.<sup>4</sup>

A second hypothesis flows from this original proposition; and it is perhaps more readily evaluated. This second thesis reflects an unique combination of ethno-methodological and labelling viewpoints. It asserts that "natural" explanations of delinquency constitute the "common meaning structure" whereby labels are attached to juveniles.

My observations suggest police and probation perspectives follow community typifications in organizing the city into areas where they expect to receive the most difficulty





from deviant or 'difficult' elements to areas where little trouble is expected and where more care should be taken in dealing with the populace because of socioeconomic and political influence (Cicourel, 1968: 67).

A test of Cicourel's thesis would be particularly useful. It would resolve the question of socio-economic and/or racial bias in police work, while at the same time testing the assumption that "natural" explanations of juvenile behaviors determine the type of police response consequent to these behaviors. Such a test is available by reference to existing research.

Our first task consists of demonstrating that natural explanations of delinquency are characteristic of police thought. It will be recalled that natural explanations of delinquency are characterized by their causal, rather than telic, foundations. Two studies, one by Nettler (1959), and a second by Wilson (1968), provide information relevant to this question. Both research efforts indicate that natural explanations do prevail in police and community conceptions of delinquency. First, Nettler's study reports the results of interviews with over 900 civic leaders, including police personnel, in one of America's largest cities. His findings reveal strong community and police preferences for causal explanations of delinquency. The second study, by Wilson, considers police in two large metropolitan departments. The two police forces, although organized along different lines (one professional, the other fraternal), are both characterized as preferring causal explanations for delinquent behaviors. In neither study is intention or purpose mentioned as a prevalent explanation for delinquency. Thus we feel safe in generalizing the finding of a preference for natural explanations of delinquency to a broad range of police departments in



North America. On this basis, it becomes possible to utilize a number of recent research efforts as a direct test of Cicourel's thesis. This thesis is, once again, that the attitudes accompanying natural explanations of delinquency result in class bias and racial discrimination in the handling of juveniles.

In evaluating the findings of the various studies of class bias and racial discrimination in police work, it will be useful to call on two sources. First, we can utilize Bordua's (1969) able summarization of past studies of the question. Second, we will report the most recent findings in this area as described in a study by Black and Reiss (1970).

Bordua has included four studies in his summarization. Each of these research efforts involved an attempt to determine the factors most often associated with police disposition of juvenile cases. The research efforts cited are those conducted by Goldman, McEachern and Bauzer, Bodine, and Terry. Bordua reaches the following conclusion regarding those factors that did influence disposition of juvenile cases in three of the four studies.

If we put together the findings of McEachern and Bauzer and of Bodine we find that offense type, arrest record, probation status, age, department, and officer all seem to affect disposition. Of the factors common to these studies and also in Goldman's, offense and previous record seem most securely established (1969: 158).

The fact that stands out in Bordua's summary of the combined results from these four different studies is that social class factors achieve no consensual recognition as influences in the final police disposition of juvenile cases.

The final study included in Bordua's summarization is a recent analysis by Ralph Terry of police work in Racine, Wisconsin. The results





of this research project are perhaps the most convincing yet considered.

. . . Terry found that offense, previous record, and age held up as correlates of disposition decision out of twelve factors studied. Terry points out that his results imply a rather "legalistic" handling of juveniles and also that the much claimed socio-economic bias of the police simply does not appear (Bordua, 1969: 158).

By way of conclusion, Bordua offers some compelling comments in explanation of the failure of class bias to show up in police statistics. The police are severely constrained in the number of juveniles that they can refer to court, and beyond the court appearance, there is very little in the way of institutional space for juveniles. Thus the police are well aware of the fact that they must return the great majority of juvenile cases to the community. As an example, in Terry's study nearly 90 per cent of the juveniles were returned to the community without a court appearance. It appears that the police must reserve the use of court referrals for only the most serious cases. Thus Bordua concludes that we should not be surprised at the failure of class bias to show up in police figures, ". . . in order for socio-economic bias to appear, it would have to be monumental since after all the police must pay some attention to the law" (Bordua, 1969: 158).

The findings reported by Black and Reiss (1970) in their most recent study allow us to focus beyond the more general problem of class bias to the possible details of racial discrimination. Again, the findings contain some surprises. Black and Reiss begin their discussion of the racial question in police work by noting that police encounters with black juveniles involve legally more serious incidents than police encounters with whites. In particular, black juveniles are far more often involved in felonies. Since in this study only 15 per cent of the



police-juvenile encounters result in arrest, the more serious nature of the offenses committed by blacks will influence the dispositional pattern of police decisions. However, the results of this study reveal that the difference in arrest rates for black and white juveniles (21 per cent for blacks, 8 per cent for whites) is not alone a consequence of the larger number of legally serious incidents that occasion police-Negro contacts.

Another major factor that influences higher arrest rates for black juveniles is whether or not a citizen complainant participates in the encounter. As Black and Reiss point out, "A complainant in search of justice can make direct demands on a policeman with which he must comply" (1970: 69). Of particular interest, in the case of complaints about black juveniles, is the fact that the complainants who seek severe dispositions are themselves black. Apparently, the white officer acting without a black complainant is considerably more lenient. Further, when no complainant is involved in the police-juvenile encounter, the racial difference in arrest rates nearly disappears (14 per cent for blacks, 10 per cent for whites).

A quote from the Reiss and Black study may help to place the whole question of police discrimination against black juveniles into its proper context.

. . . it is evident that the higher arrest rate for Negro juveniles in encounters with complainants and suspects is largely a consequence of the tendency of the police to comply with the preferences of complainants. . . . Given the prominent role of the Negro complainant in the race differential, then, it may be inappropriate to consider this pattern an instance of discrimination on the part of policemen. While police behavior follows the same patterns for Negro and White juveniles, differential outcomes arise from differences in citizen behavior (1970: 71-72).





The findings of this study lead us to what will be an unpopular conclusion among many sociologists. This conclusion is that the police apparently do not demonstrably discriminate according to race in their handling of juvenile cases.

The inescapable result of our empirical test seems to be a verdict of rejection for Cicourel's hypothesis. The prevalence of natural explanations for delinquency does not seem to influence the manner in which the police exercise their discretion in the handling of juveniles. Perhaps we should not be surprised. Disparity between attitudes and actions is a recurring theme in the human experience; and at least in this case, we are the better for it.

We have considered an unusual and important instance where ethnomethodology and labelling theory have combined to formulate a falsifiable hypothesis. An alleged invariant property of interaction--typification--and a central tenet of labelling theory--socio-economic bias--have both been discredited in their application to the study of deviance. One would like to venture the observation that the neo-Chicagoans' new clothes may be transparent. But the garments in this explanatory tale contain their own intrinsic charm; and naked of empirical reality or not, the dictates of fashion demand their moment of acceptance. Moynihan provides an apt description of our condition:

. . . there is. . . the tyranny of fashion: a mysterious force, but an open enough one. Fashions of thought get set, and for a period at least they prevail. Evidence to the contrary is treated not as information but as wrongdoing, and woe betide the bearer of such news (1970: 96-97).





## CONCLUSIONS

We have analyzed in considerable detail two methodologies, both prevalent in contemporary deviance analysis. These methods have been found to encourage naïve and promiscuous conceptualization. What remains of our task is to establish the importance of these findings.

Ethno-methodology and analytic induction are both outgrowths of a thoughtway more recently known as naturalism. We have seen that naturalism places a sometimes fruitful emphasis on man's subjective experience of empirical reality. Naturalism further instructs us that to transcend this subjective bias we must pursue the 'essence' of subjective experience. Our discovery of essence is thought to be signalled by (1) the successful formation of universal concepts or by (2) the derivation of invariant properties.

Although the above assumptions may by now seem of obvious disutility, they remain of considerable importance--for naturalism shares a symbiotic relationship with symbolic interactionism. The method is so vitally linked with the theoretical perspective that if one is judged invalid--then surely the other must also be travelling in dangerous waters. This is an important conclusion, and its acceptance demands that the link between symbolic interactionism and the naturalist methodologies be demonstrated.

The symbiotic link between the naturalist thoughtway and symbolic interactionism can be seen in the early writings of George Herbert Mead. In spite of the fact that his formulation of symbolic interactionism placed a heavy emphasis on the subjective experience, Mead recognized that extreme subjectivism carried the risk of reducing all knowledge to



a matter of personal experience. To avoid this epistemological dead-end, Mead assumed man's capacity to transcend the subjective experience via 'reflective consciousness.'

We recognize here three striking results of the development of reflective consciousness in the modern world: first, it is assumed that the objective world of knowledge can be placed within the experience of the individual without losing thereby its nature as an object, that all characters of that object can be presented as belonging to that experience, whether adequately or not is another question; and second, it is assumed that the contradictions in its nature which are associated with its inclusion in individual experience, its reference beyond itself when so included, may themselves be the starting-point of a reconstruction which at least carries that object beyond the experience within which these contradictions arose; and third, it is assumed that this growth takes place in a world of reality within which the incomplete experience of the individual is an essential part of the process, in which it is not a mere fiction, destroying reality by its representation, but is a growing-point in that reality itself (Mead, 1917: 197).

Mead finds in this image of a reflective consciousness all the necessary elements for a 'conscious method.' This conscious method, in turn, allegedly forms the basis of modern science.

But there is one additional element of the conscious method which must be described before we will have completed Mead's picture of science. The final element is, of course, the requirement of universality--or invariance, as moderns may wish to call it.

In discovery, invention, and research the escape from the exceptional, from the data of early stages of observation, is by the way of an hypothesis; and every hypothesis so far as it is tenable and workable in its form is universal. No one would waste his time with a hypothesis which confessedly was not applicable to all instances of the problem (Mead, 1917: 209).

Mead goes on to argue that the unceasing quest for universals, under the constant stimulation of exceptional cases, is the scenario





for scientific advancement.

The reader will recognize that all of these assumptions have been disputed in earlier stages of our discussion. Our effort here has been directed at connecting the viewpoint of naturalism with the perspective of symbolic interactionism. The link has been established by reference to the common source of both thoughtways--the writings of George Herbert Mead.

Our conclusion is quite simple. Having discounted the naturalist methodologies, and having established their symbiotic relationship to symbolic interactionism, we find the perspective lacking in techniques of verification.<sup>5</sup> The risk of such a condition is that claims to knowledge soon become dependent on empathetic appeal. And while we all appreciate a good story, we also aspire to known facts.



## FOOTNOTES

<sup>1</sup>The assumption that direct involvement with the phenomenon to be explained is productive of accurate knowledge is as yet untested.

<sup>2</sup>The assumption that concept determines percept is ancient. For a rebuttal of this argument see Nettler (1970 : 92-94).

<sup>3</sup>Exceptions to Cressey's fourth hypothesis are found in a test of the embezzlement hypothesis by Nettler (1970a).

<sup>4</sup>Only one possible exception to this hypothesis is immediately apparent: Matza's attribution of free will to delinquent behaviors (1964; 1969).

<sup>5</sup>Certainly the methodologies I have considered in this discussion are not the only ones associated with symbolic interactionism. However, it is the author's contention that the naturalist methods here considered most accurately reflect the epistemological assumptions inherent in the interactionist thoughtway. For a broader discussion inclusive of attitudinal research methods, see Nettler's (1970b: 33-85) discussion of symbolic interactionism.



## CHAPTER FIVE

### THE HERESY OF HUMANISM

What follows is an attempt to reach conclusions regarding Psychologies Exposed. The format to be followed consists of a series of three questions and their respective answers. Each question intends an important comment on the critical perspective utilized in the preceding discussions.

COMMENT 1: HASN'T THIS STUDY SIMPLY RESURRECTED THE GHOST OF POSITIVISM TO SLAY THE SPIRIT OF HUMANISM AND, IN THE PROCESS, CREATED ITS OWN EPISTEMOLOGICAL MONSTER?

The answer to this question will benefit from the placement of symbolic interactionism in its historical context.

It is a continuing paradox that the great teachers are often contradicted by their students. Thus while Mead demonstrated a qualified respect for positivism with his pragmatic attempt to apply the scientific method to philosophical problems (Shibutani, 1968: 84; Mead, 1917), his students have often expressed disdain for the prevailing scientific thoughtways (Matza, 1969: 3-14). More specifically, where Mead sought to realize a counterpart to the scientific method in human consciousness, reflective intelligence, Mead's students have attempted to use the same vision of "mind" to demonstrate the disutility of a method called science. Theoretical afterlife, it seems, brings mutations--if not outright distortions. Mead warned of such historical tricks of fate when he advised that ". . . the novelty of every future demands





a novel past" (1934b: 337). Nevertheless, the recent epistemological change of pace by the interactionists is alarming.

It is perhaps on a point of similarity, therefore, that we begin, like Mead, with respect for the scientific method. If this should make us positivists or creators of epistemological monsters, then that we are. Before reaching conclusions, however, some differences of opinion with Mead should be recognized. Mead remained firmly convinced that application of the scientific method to the study of social problems promised progress towards their solution. Our picture is not so sanguine. We recognize that science will not save us (Nettler, 1970b: 203), even though its method may be an ameliorant for certain types of problems. On the other hand, we remain more firmly committed than Mead to the importance of direct and independent measures of phenomena. For example, we have held that an hypothesis that links thought to behavior as cause to effect must specify the independent and observable criteria of each. Thus, while the first of our differences would seem to make us less the positivist than Mead, the second would seem to connote the opposite. The reader is left to his own conclusions.

Perhaps of more importance is the contradiction of Mead by his followers. Mead argued, as we have indicated before, that man's capacity for "reflective consciousness" enables him progressively to improve his approximate knowledge of an objective reality. Mead characterized this "conscious method" as the foundation of modern science (1917: 197). In contrast, Becker (1967: 241-47) demonstrates impatience with his mentor's approach when he beseeches us to decide "Whose Side Are We On?" Where Mead suggested the reflective consideration of a variety of



viewpoints, steadily progressing towards some objective resolution of discrepancies, Becker apparently rejects such a procedure.

The point is obvious. By pursuing this seemingly simple solution, we arrive at a problem of infinite regress. . . there is no end to it and we can never have a "balanced picture" until we have studied all of society simultaneously. I do not propose to hold my breath until that happy day (1967: 247).

Becker's conclusion is that "We can never avoid taking sides" (1967: 245) so that we might just as well ". . . take sides as our personal and political commitments dictate. . ." (1967: 247).

While we can agree with neither Mead (see Chapter Four) nor Becker, there is an important difference. Mead's version of science suffers largely an infection of empathy. In seeking to understand, feel, or take on the role of the other, Mead asks us to substitute intuition for knowledge. Becker, on the other hand, compounds the error of empathy with a liberal admixture of ideology. In urging us to "take sides," Becker adds evaluative and political dimensions to our studies--and these can be interpreted as warning signals of incipient ideology. Once having chosen sides, the researcher can easily rationalize a diminished interest in verificational procedures, and it is then that the masquerade of valued belief as known fact can begin in full. Thus while Becker may have succeeded in stalling the "infinite regression," he has accomplished his goal at the potential cost of a diminished concern for the factual.

Our preference for facts leads us to reject Becker's call to arms, and we conclude by inference that Mead would have seconded such a rejection. But what, then, is the common link between Becker and Mead?





At least one thread of consensus that ties the inconsistencies of the Meadian tradition together, while at the same time providing some semblance of explanation for its ironies, is the persistent loyalty of the Meadians to a humanistic social philosophy. Blumer and Matza have described the humanistic leanings of the Meadian tradition.

Blumer: Symbolic interactionism provides the premises for a profound philosophy with a strong humanistic cast. In elevating the 'self' to a position of paramount importance. . . , symbolic interactionism provides the essentials for a provocative philosophical scheme that is peculiarly attuned to social experience. The outlines of this philosophy are sketched especially in the writings of George Herbert Mead. . . (1969: 21).

Matza: The Meadian view is in large measure the sociological tradition that maintained the humanist stress on subjectivity. He was not alone in that endeavor but, perhaps, central in the United States (1969: 7).

It seems that where faith in the utility of the scientific method divides Meadians, a fundamental concern for the dignity of human existence reunites them. Thus almost all advocates of the symbolic interactionist perspective place important emphasis on the qualities that distinguish man from the lower forms of life. Also present is a stubborn faith in the potential for progressive refinement, or at least redemption, of these superior human qualities. Finally, it is man's phenomenological experience of and through these distinctively human qualities that the interactionists seek to understand in their efforts at explanation.

Each of the above concerns produces its own problems; however, it is the last of these concerns that we will deal with here. In focussing his explanatory efforts on man's phenomenological or subjective experience of his existence, Mead confounded a fundamental requirement of



scientific theory construction. This requirement states that independent criteria must be provided for the measurement of separate variables; the conceptual scheme suggested by Mead fails this requirement in two ways. First, Mead's scheme denies any conceptual independence of variables and assumes that whatever overt manifestation these variables may have must be the result of a complex and continuous process of interaction. The intricately interwoven details of a causal web this diffuse defy the logician's measure--both in quantity and quality (cf. Blumer, 1956: 683-90). Second, Mead suggested a willingness to utilize a single overt measure as indicator for not only a behavioral tendency, but also for a larger underlying thought pattern (cf. Mead, 1934: 121-22). Mead's students have demonstrated a fondness for following this suggestion (cf. Chapter One of this thesis). As a consequence of both factors discussed, a "theoretical" system emerges that conveys the uneasy image of intentional circularity. An implication is the clear and present danger of tautology.

COMMENT II: PHILOSOPHICAL PRECONCEPTIONS NOTWITHSTANDING,  
ISN'T IT POSSIBLE THAT LIFE IS A GRAND TAUTOLOGY?

There is heresy associated with the methodological implications of symbolic interactionism, and Matza has argued that this ". . . heresy of humanism must be made explicit" (1969: 109). Promoting an epistemology thoughtway known as "naturalism," Matza suggests that a first step towards the heresy of humanism is the questioning of those standards characteristic of the philosophy of science. "The commitment of naturalism," Matza advises us, ". . . is to phenomena and their nature; not to Science or any other system of standards" (1969: 3). Further,





. . . in the study of man, there is no antagonism between naturalism and a repudiation of the objective view, nor a contradiction between naturalism and the humane methods of experience, reason, intuition, and empathy. Naturalism in the study of man is a disciplined and rigorous humanism (1969: 8).

His foundation set, Matza next very casually introduces a rationalization for conceptual ambiguity in the study of 'deviance.'

Students of society must tolerate such ambiguity. Finely drawn and strictly operational definitions leaving no place for ambiguity may be a source of satisfaction for the analyst, but he will find that ordinary subjects of inquiry have the capacity to subvert such conceptions and render them useless. . . . the clear-cut yes or no will be gained only by suppressing and thus denying, the patent ambiguity of this novel phenomenon and the easily observable tentative, vacillating, and shifty responses to it. Accordingly, the cost of rigor may be deemed excessive. . . (1969: 11).

Matza seems to be saying that a little bit of ambiguity never damaged a theory, and that such conceptualization is needed to mirror reality anyway. But overlapping concepts contain the threat of tautology. And carried to its extreme, this viewpoint seems to argue that life is, after all, a grand tautology--one thing running over into the next, and no thing maintaining its own independent existence. There are difficulties with this perspective, particularly as applied to 'deviance.'

If we are to concede that much of life is ambiguous, and if we further decide to study this aspect of "reality," then our explanations may, as we have indicated, be infected with the problems of tautology. A characteristic of such explanations is that they multiply concepts, one to explain the other, where each is an indicator of the same very general realm of reality. Overlapping and ambiguous concepts are, then, deceptive. They satisfy our curiosity and evoke feelings of





understanding by redefining the thing to be explained in terms of itself.

The linguistic legerdemain through which we confound definitions with propositions can satisfy us with an 'understanding' that is vacant. . . . This is another way of saying that it satisfies curiosity with nonempirical utterances, irrefutable because nonpropositional, plausible, but unpredictable (Nettler, 1970b: 71).

A major detraction of tautological explanations is, as Nettler indicates, their inability to predict. But Matza seeks different goals.

Like many other deviance analysts who utilize the interactionist perspective, Matza aspires to an "appreciation" of deviance. The positivist's mistake, Matza would tell us, is the desire to correct rather than to appreciate the deviant. Such feelings interfere with the capacity to empathize and thus comprehend the subject of inquiry (1969: 15).

These appreciative sentiments are easily summarized: we do not for a moment wish that we could rid ourselves of deviant phenomena. We are intrigued by them. They are an intrinsic, ineradicable, and vital part of human society (1969: 17).

One can infer from Matza's comments that tautology is no major threat to the appreciation of deviance. The aesthetic obsolescence of attempts to control deviance renders prediction unnecessary.

Couldner has registered at least one serious criticism of the interactionist's romantic attitudes.

It expresses the satisfaction of the Great White Hunter who has bravely risked the perils of the urban jungle to bring back an exotic specimen. It expresses the Romanticism of the zoo curator who preeningly displays his rare specimens . . . . The attitude of these zookeepers of deviance is to create a comfortable and humane Indian Reservation, a protected social space within which these colorful specimens may be exhibited, unmolested and unchanged (1968: 106).

But the problems of deviance are larger than the appreciative interactionist would like to believe. And more than this, the attitudes of the interactionists demonstrate a good deal of inconsistency--



sometimes indicating satisfaction with the role of appreciative curator, but at other times revealing aspirations to the role of humanitarian agent of change. Like it or not, then, the theoretical capacity for prediction and control remains of some importance. Tautological explanations evade the test of prediction with porous and infallible concepts. Therapies based on such explanations offer little hope for change, control, or "help"; one is tempted to conclude that the heresy of humanism offers a revolution without revelations.

There is an alternative to the heresy of humanism. It recognizes that tautology is a part of the human experience, but it places this fact in a different context. We begin with the premise that tautology is one expression of man's innate ability to conceptualize. Its basis is the physiological structure of the mind. Our cognitive processes allow us to represent concepts with words (one variety of symbol), and thereby to relate things. We observe that any word can represent many things, while more than one word can also represent the same thing. These ambiguities are built into our mental processes; it is therefore not surprising that when we try to relate one thing to another we are often confounded by our words. Tautology occurs, then, when our words overlap in their references--more specifically, when they refer to the same thing. Much in our experience that is ambiguous is "known" as tautology. That which can be made specific in our experience may often avoid the pitfalls of tautology. Our business in social science is to expand on this latter aspect of our experience.<sup>1</sup> While such an approach may limit the aspects of reality that we can adequately explain, at the same time it provides the hope that applications of our knowledge will





involve more than appreciation and description. It may amount to the difference between art and science.

COMMENT III: THE PRESENT CRITIQUE OF SYMBOLIC INTERACTIONISM REFLECTS THE VALUES OF ESTABLISHMENT SOCIOLOGY. DOESN'T THIS VIEWPOINT SUFFER A CONSERVATIVE BIAS?

Sartre tells us that, "First of all, I do not believe that one can be an intellectual without being left wing" (1970: 52). Similarly, Gouldner tells us that a "new sociology" must be radical.

Radical, because it would recognize that knowledge of the world cannot be advanced apart from the sociologist's knowledge of himself and his position in the social world, or apart from his efforts to change these. Radical, because it seeks to transform as well as to know the alien world outside the sociologist as well as the alien world inside of him. Radical, because it would accept the fact that the roots of sociology pass through the sociologist as a total man, and that the question he must confront, therefore, is not merely how to work but how to live (1970: 489).

In telling us that to be "right" you must carry your sentiments on the "left," Sartre and Gouldner seem to be promoting something of a genetic fallacy. Equating the accuracy of a viewpoint with the location of its source encourages the emergence of argumenta ad hominem and promises nothing towards the determination of truth. But this may be the lesser of the tolls exacted by a new sociology. For what Gouldner recommends to old sociology is an entirely new praxis. This praxis begins with the requirement of a new life style.

In the last analysis, if a man wants to change what he knows he must change how he lives; he must change his praxis in the world (1970: 493).

There are additional requirements of those who will be "chosen" to serve the "new mission for sociology."



Gouldner's scheme for achieving the new praxis involves at least three additional prerequisites. The first of these is "normative objectification," referring to the capacity to distinguish the "side" to which one is attached from the grounds on which one is attached to it. The fundamental dictate of this requirement is that the sociologist must not deceive others concerning the value basis of his judgments. The second factor in achieving the new praxis is "personal authenticity," referring to the capacity for admitting the factuality of things that violate one's own hopes and values. The dictate associated with this requirement is that the sociologist not deceive himself concerning the basis of his judgments. The final factor associated with the new praxis is the most conventional, "transpersonal replicability," referring to the requirement that one describe his procedures completely so that others may employ them in testing for the same results (1968: 112-14).

None of Gouldner's requirements for a new praxis evoke much surprise until they are combined with the call for a new commitment to values by the sociologist.

. . . men's highest values, no less than their basest impulses, may make liars of them. Nonetheless, a Reflexive Sociology accepts the dangers of a value commitment, for it prefers the risk of ending in distortion to beginning in it, as does a dogmatic and arid value-free sociology (1970: 499).

With this reference to values, Gouldner introduces the argument that all of sociology has ideological overtones and that his ideology provides a more reliable avenue to knowledge than the older thoughtways. Like all truth-tellers, Gouldner "knows" that his truth is the more valid. The danger is that the "wanting" associated with Gouldner's value commitment will corrupt the verificational procedures necessary for "knowing."





There is inferential evidence of such a liability in Gouldner's discussions.

. . . questions of fact--that is, concern with what the facts are--seem to enter surprisingly little into much social theory; at any rate, they seem to have far less importance for theory-making than the methodologists and logicians of science suggest (1970: 483).

It often seems that the making of social theory can get underway, and be sustained, only when questions of fact are deferred or ignored (1970: 484).

The important issue is not the determination of facts, but rather the ordering of them (1970: 484).

Gouldner suggests that rather than expend our efforts in the clarification of facts and the verification of factual relationships, we should, instead, assume them through the medium of personal experience. "Social theorizing, then, is often a search for the meaning of the personally real, that which is already assumed to be known through personal experience" (1970: 484). With this conclusion, we return full circle to the problem of subjectivity posed earlier by George Herbert Mead.

Perhaps it will be useful to summarize the steps we have followed since first introducing the problem of subjectivity in Chapter Four. We noted that Mead saw information gained through personal experience as moving from the subjective to the objective by means of a human capacity known as "reflective consciousness." We argued that reflective consciousness formed the basis for a verificational scheme later known as analytic induction. A number of logical flaws were observed in the method of analytic induction. Ethno-methodology was then characterized as another "way of knowing" with its origin in the work of Mead. We observed that this approach to the empirical world also contained a number of serious problems. Another recent solution to the problem of subjectivity was





considered in Becker's suggestion that we "take sides." Here we agreed with Gouldner's criticisms of this approach and we concluded that Becker's solution contained the seeds of ideology. Finally, in this last section we described Gouldner's recommendations that we adopt a new ideology known as "reflexive sociology." We criticized this "solution" as diminishing concern for the factual; further, it would seem to be a "solution" that does little more than state in new terms the original problem of subjectivity as formulated by Mead.

If we are to extract ourselves from this epistemological encirclement, we must realize a more useful solution that gives notice to both halves of a perceptual paradox. The first half of this paradox consists of the fact that values, like other conceptual artifacts, sometimes color our perceptions. Such distortion most often occurs when the stimulus-to-be-perceived is ambiguous--i.e., poorly defined and abstract. The second half of the paradox tells us that not all perceptions are tinged by concepts and that fact does, therefore, have an objective existence. This objectivity is more easily achieved when the stimulus-to-be-perceived is unambiguous--i.e., specifically defined and concrete. The realization of this perceptual paradox in whole alerts us to the use and purpose of verificational procedures.

It is proposed, in short, that we adopt a solution whose goal is a scientific sociology. This solution realizes that many of our explanations will by default find their expression in art and philosophy, their slant being humanistic, and their function being the satisfaction of curiosity. We are aware, in other words, that not all varieties of "knowledge" are testable in the scientific sense. Nonetheless, this



same solution suggests that the sociology called social science can play a more distinctive role in providing knowledge characterized as fact. Nettler has suggested the following functions to be served by such a sociology:

- the as-yet neglected task of cataloguing what people "really do."
- ascertaining relationships among. . . beliefs and behaviors.
- the development of methods for counting the consequences of public policy.
- the inchoate possibility of forecasting events.
- the dissemination of information concerning more efficient ways of using our heads (1968: 205).

In the end, the solution suggested carries its own share of humility. It is an accommodation born of moderation: admitting that not all aspects of life are knowable nor all problems soluble. In at least these ways it varies from the naivety of an interactionist or reflexive sociology. But more importantly, the solution suggested points to the fact that most phenomena are capable of being studied for their factual content; and it is to these phenomena that the tools of sociology as science are usefully applied. In the study of deviance, no less than in the other sub-fields of sociology, this will also be the case.





## FOOTNOTES

<sup>1</sup>Wittgenstein's (1961: 67-71; 129) discussion of tautology and contradiction seems to express the point we are trying to make. Anscombe (1959: 77) summarizes Wittgenstein's discussion as follows:

We can now understand some of what Wittgenstein says about tautology and contradiction. They are not 'pictures'. . . , just as all-white or all-black globes are not maps. And so they are not 'logical connections of signs'. . . : the relations between them are non-significant--i.e., depict nothing: the representing relations, like two projections which between them fill a space, cancel one another out.

The all-white globe, though, might be said to be a representation of the whole world. It is because of the shape of the whole that the two shapes, p together with not-p, combine to make the shape of the whole. And this throws light on what Wittgenstein means when he says that the logical propositions describe, or rather represent, the framework of the world. 'It must shew something, that certain combinations of symbols are tautologies.' But what is represented here is not something that 'we express by means of the signs,' but that 'speaks out on its own account'. . . ."



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